

RECEIVED MAY 22 1932

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MATHEMATICAL GAZETTE

EDITED BY
T. A. A. BROADBENT, M.A.

LONDON
G. BELL & SONS, LTD., PORTUGAL STREET, KINGSWAY, W.C.2

Vol. XVI., No. 213.

MAY, 1932.

4s. Net.

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INTERNATIONAL CONGRESS OF MATHEMATICIANS,
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THE programme of the forthcoming International Congress of Mathematicians, which is to be held at Zurich in September, has now appeared, and makes attractive reading.

The Reception of members of the Congress will take place on 4th September, and the Inaugural Meeting the following morning. The afternoon of 6th September and the whole of 8th September are being kept free for excursions. The final lecture will be delivered on the morning of 12th September.

A list of those who are to deliver lectures will perhaps be of use in indicating the scope and interest of the Congress : J. W. Alexander, S. Bernstein, L. Bieberbach, H. Bohr, C. Carathéodory, T. Carleman, E. Cartan, R. Fueter, G. H. Hardy, G. Julia, K. Menger, M. Morse, R. Nevanlinna, E. Noether, W. Pauli, F. Riesz, F. Severi, W. Sierpinski, J. Stenzel, N. Tschebotarow, G. Valiron and R. Wavre.

Ten sections are provided, and sectional meetings will be held on four afternoons. Members of the Mathematical Association will note with interest that a meeting of the International Commission for the Teaching of Mathematics will take place during the Congress, and that an afternoon will be reserved for it in Section 10 (Pedagogy).

Apart from the excursions on 6th and 8th September, it is hoped that after the Congress an excursion will be arranged to the Jungfrau joch, with a visit to the scientific station there.

The programme also gives much useful information about the nature and cost of hotel accommodation in Zurich and about the arrangements whereby the members of the Congress can secure a reduction in the cost of travelling in Switzerland. Further information on any point connected with the Congress can be obtained by writing to :

The International Congress of Mathematicians, Zurich.
Ecole Polytechnique Fédérale, Salle 20d.

THE DECLINE OF DETERMINISM.

PRESIDENTIAL ADDRESS TO THE MATHEMATICAL ASSOCIATION, 1932.

BY SIR ARTHUR EDDINGTON, F.R.S.

DETERMINISM has faded out of theoretical physics. Its exit has been commented on in various ways. Some writers are incredulous and cannot be persuaded that determinism has really been eliminated. Some think that it is only a domestic change in physics, having no reactions on general philosophic thought. Some imagine that it is a justification for miracles. Some decide cynically to wait and see if determinism fades again.

The rejection of determinism is in no sense an abdication of scientific method ; indeed it has increased the power and precision of the mathematical analysis of observed phenomena. On the other hand I cannot agree with those who belittle the general philosophical significance of the change. The withdrawal of physical science from an attitude it has adopted consistently for more than 200 years is not to be treated lightly ; and it involves a reconsideration of our views with regard to one of the perplexing problems of our existence. In this address, I shall deal mainly with the physical universe, and say very little about mental determinism or freewill. That might well be left to those who are more accustomed to arguing about such questions if only they could be awakened to the new situation which has arisen on the physical side. At present I can see little sign of such an awakening. Waking is a rude process ; and if I sometimes shout it is because current literature resounds with the snores of those who are asleep.

Definitions of Determinism.

Let us first be sure that we agree as to what is meant by determinism. I quote three definitions or descriptions for your consideration. The first is by a mathematician (Laplace) :

We ought then to regard the present state of the universe as the effect of its antecedent state and the cause of the state that is to follow. An intelligence, who for a given instant should be acquainted with all the forces by which nature is animated and with the several positions of the entities composing it, if further his intellect were vast enough to submit those data to analysis, would include in one and the same formula the movements of the largest bodies in the universe and those of the lightest atom. Nothing would be uncertain for him ; the future as well as the past would be present to his eyes. The human mind in the perfection it has been able to give to astronomy affords a feeble outline of such an intelligence. . . . All its efforts in the search for truth tend to approximate without limit to the intelligence we have just imagined.

The second is by a philosopher (C. D. Broad) :

“ Determinism ” is the name given to the following doctrine. Let S be any substance, ψ any characteristic, and t any moment. Suppose that S is in fact in the state σ with respect to ψ at t . Then the compound

supposition that everything else in the world should have been exactly as it in fact was, and that S should have been in one of the other two alternative states with respect to ψ is an impossible one. [The three alternative states (of which σ is one) are: to have the characteristic ψ , not to have it, and to be changing.]

The third is by a poet (Omar Khayyam) :

With Earth's first Clay They did the Last Man's knead,
And then of the Last Harvest sow'd the Seed :
Yea, the first Morning of Creation wrote
What the Last Dawn of Reckoning shall read.

I propose to take the poet's description as my standard. Perhaps you will think this an odd choice ; but there is no doubt that his words express what is in our minds when we refer to determinism. The other two definitions need to be scrutinised suspiciously ; we are afraid there may be a catch in them. In saying that the physical universe as now pictured is not a universe in which "the first morning of creation wrote what the last dawn of reckoning shall read", we make it clear that the abandonment of determinism is no technical quibble but is to be understood in the most ordinary sense of the word.

It is important to notice that all three definitions introduce the time-element. Determinism postulates not merely causes but *pre-existing* causes. Determinism means predetermination. Hence in any argument about determinism the dating of the alleged causes is an important matter ; we must challenge them to produce their birth certificates.

Ten years ago practically every physicist of repute was, or believed himself to be, a determinist, at any rate so far as inorganic phenomena are concerned. He believed that he had come across a scheme of strictly causal law, and that it was the primary aim of science to fit as much of our experience as possible into such a scheme. The methods, definitions, and conceptions of physical science were so much bound up with this assumption of determinism that the limits (if any) of the scheme of causal law were looked upon as the ultimate limits of physical science.

To see the change that has occurred, we can consider a recent book which goes as deeply as anyone has yet penetrated into the fundamental structure of the physical universe, Dirac's *Quantum Mechanics*. I do not know whether Dirac is a determinist or not ; quite possibly he believes as firmly as ever in the existence of a scheme of strict causal law. But the significant thing is that in this book he has no occasion to refer to it. In the fullest account of what has yet been ascertained as to the way things work, causal law is not mentioned.

This is a deliberate change in the aim of theoretical physics. If the older physicist had been asked why he thought that progress consisted in fitting more and more phenomena into a deterministic scheme his most effective reply would have been "What else is there to do ?" A book such as Dirac's supplies the answer. For the new aim has been extraordinarily fruitful, and phenomena which had

hitherto baffled exact mathematical treatment are now calculated and the predictions are verified by experiment. We shall see presently that indeterministic law is as useful a basis for practical predictions as deterministic law was. By all practical tests progress along this new branch track must be recognised as a great advance in knowledge. No doubt some will say "Yes, but it is often necessary to make a detour in order to get round an obstacle. Presently we shall have passed the obstacle and be able to join the old road again". I should say rather that we are like explorers on whom at last it has dawned that there are other enterprises worth pursuing besides finding the North-West Passage; and we need not take too seriously the prophecy of the old mariners who regard these enterprises as a temporary diversion to be followed by a return to the "true aim of geographical exploration". But at the moment I am not concerned with prophecy and counter-prophecy; the important thing is to grasp the facts of the present situation.

Secondary Law.

Let us first try to see how the new aim of physical science originated. We observe certain regularities in the course of nature and formulate these as laws of nature. Laws may be stated positively or negatively, "Thou shalt" or "Thou shalt not". For the present purpose it is most convenient to formulate them negatively. Consider the following two regularities which occur in our experience:

- (a) We never come across equilateral triangles whose angles are unequal.
- (b) We never come across thirteen trumps in our hand at bridge.
- ~ In our ordinary outlook we explain these regularities in fundamentally different ways. We say that the first occurs because the contrary experience is *impossible*; the second occurs because the contrary experience is *too improbable*.

This distinction is entirely theoretical; there is nothing in the observations themselves to suggest which type a particular regularity belongs to. We recognise that "impossible" and "too improbable" can both give adequate explanation of any observed uniformity of experience, and the older theory rather haphazardly explained some uniformities one way and other uniformities the other way. In the new physics we make no such discrimination; the union obviously must be on the basis of (b) not (a). It can scarcely be supposed that there is a law of nature which makes the holding of thirteen trumps in a properly dealt hand impossible; but it can be supposed that our failure to find equilateral triangles with unequal angles is only because such triangles are *too improbable*. Of course my remark does not refer to the theorem of pure geometry; I am speaking of regularities of our experience and refer therefore to the experience which is supposed to confirm this property of an equilateral triangle as being true of actual measurement. Our measurements regularly confirm it to within the highest accuracy attainable and no doubt will always do so; but according to modern theory that is because

a failure could only occur as the result of an exceedingly improbable coincidence in the behaviour of the vast number of particles concerned in any experimental measurement.

We must, however, first consider the older view which distinguished type (a) as a special class of regularity. Accordingly there were two types of natural law. The earth keeps revolving round the sun because it is *impossible* it should run away. Heat flows from a hot body to a cold because it is *too improbable* that it should flow the other way. I call the first type *primary* law, and the second type *secondary* law. The recognition of secondary law was the thin end of the wedge that ultimately cleft the deterministic scheme.

For practical purposes primary and secondary law exert equally strict control. The improbability referred to in secondary law is so enormous that failure even in an isolated case is not to be seriously contemplated. You would be utterly astounded if heat flowed from you to the fire so that you got chilled by standing in front of it, although such an occurrence is judged by physical theory to be not impossible but improbable. Now it is axiomatic that in a deterministic scheme nothing is left to chance ; a law which has the ghost of a chance of failure cannot form part of the scheme. So long as the aim of physics is to bring to light a deterministic scheme, the pursuit of secondary law is a blind alley since it leads only to probabilities. The determinist is not content with a law which prescribes that, given reasonable luck, the fire will warm me ; he admits that that is the probable effect, but adds that somewhere at the base of physics there are other laws which prescribe just what the fire will do to me, luck or no luck.

To borrow an analogy from genetics, determinism is a *dominant character*. We can (and indeed must) have secondary indeterministic laws within any scheme of primary deterministic law—laws which tell us what is likely to happen but are overridden by the dominant laws which tell us what must happen. So determinism watched with equanimity the development of indeterministic law within itself. What matter ? Deterministic law remains dominant. It was not foreseen that indeterministic law when fully grown might be able to stand by itself and supplant its dominant parent. There is a game called "Think of a number". After doubling, adding, and other calculations, there comes the direction "Take away the number you first thought of". We have reached that position in physics, and the time has come to take away the determinism we first thought of.

The growth of secondary law within the deterministic scheme was remarkable, and gradually sections of the subject formerly dealt with by primary law were transferred to it. There came a time when in some of the most progressive branches of physics secondary law was used exclusively. The physicist might continue to profess allegiance to primary law but he ceased to utilise it. Primary law was the gold to be kept stored in vaults ; secondary law was the paper to be used for actual transactions. No one minded ; it was taken for granted that the paper was backed by gold. At last came

the crisis and *physics went off the gold standard*. This happened very recently and opinions are divided as to what the result will be. Professor Einstein, I believe, fears disastrous inflation and urges a return to sound currency—if we can discover it. But most theoretical physicists have begun to wonder why the now idle gold should have been credited with such magic properties. At any rate the thing has happened and the immediate result has been a big advance in atomic physics.

We have seen that indeterministic or secondary law accounts for regularities of experience, so that it can be used for predicting the future as satisfactorily as primary law. The predictions and regularities refer to average behaviour of the vast number of particles concerned in most of our observations. When we deal with fewer particles the indeterminacy begins to be appreciable, and prediction becomes more of a gamble ; till finally the behaviour of a single atom or electron has a very large measure of indeterminacy. Although some courses may be more probable than others, backing an electron to do anything is in general as uncertain as backing a horse.

It is commonly objected that our uncertainty as to what the electron will do in the future is due not to indeterminism but to ignorance. It is asserted that some character exists in the electron or its surroundings which decides its future, only physicists have not yet learned how to detect it. You will see later how I deal with this suggestion. But I would here point out that if the physicist is to take any part in the wider discussion on determinism as affecting the significance of our lives and the responsibility of our decisions, he must do so on the basis of what he has discovered, not on the basis of what it is conjectured he might discover. His first step should be to make clear that he no longer holds the position, occupied for so long, of chief advocate for determinism, and that if there is any deterministic law in the physical universe *he is unaware of it*. He steps aside and leaves it to others—philosophers, psychologists, theologians—to come forward and show, if they can, that they have found indications of determinism in some other way.* If no one comes forward the hypothesis of determinism presumably drops ; and the question whether physics is actually antagonistic to it scarcely arises. It is no use looking for an opposer until there is a proposer in the field.

Inferential Knowledge.

It is now necessary to examine rather closely the nature of our knowledge of the physical universe.

* With a view to learning what might be said from the philosophical side against the abandonment of determinism, I took part in a symposium of the Aristotelian Society and Mind Association in July, 1931. Indeterminists were strongly represented, but unfortunately there were no determinists in the symposium, and apparently none in the audience which discussed it. I can scarcely suppose that determinist philosophers are extinct, but it may be left to their colleagues to deal with them.

All our knowledge of physical objects is by inference. Our minds have no means of getting into direct contact with them ; but they emit and scatter light waves, and they are the source of pressures transmitted through adjacent material. They are like broadcasting stations that send out signals which we can receive. At one stage of the transmission the signals pass along nerves within our bodies. Ultimately visual, tactal and other sensations are provoked in the mind. It is from these remote effects that we have to argue back to the properties of the physical object at the far end of the chain of transmission. The image which arises in the mind is not the physical object, though it is a source of information about the physical object ; to confuse the mental object with the physical object is to confuse the clue with the criminal. Life would be impossible if there were no kind of correspondence between the external world and the picture of it in our minds ; and natural selection (reinforced where necessary by the selective activity of the Lunacy Commissioners) has seen to it that the correspondence is sufficient for practical needs. But we cannot rely on the correspondence, and in physics we do not accept any detail of the picture unless it is confirmed by more exact methods of inference.

The external world of physics is thus a universe populated with *inferences*. The inferences differ in degree and not in kind. Familiar objects which I handle are just as much inferential as a remote star which I infer from a faint image on a photographic plate or an "undiscovered" planet inferred from irregularities in the motion of Uranus. It is sometimes asserted that electrons are essentially more hypothetical than stars. There is no ground for such a distinction. By an instrument called a Geiger Counter electrons may be counted one by one as an observer counts one by one the stars in the sky. In each case the actual counting depends on a remote indication of the physical object. Erroneous properties may be attributed to the electron by fallacious or insufficiently grounded inference, so that we may have a totally wrong impression of what it is we are counting ; but the same is equally true of the stars. The rules of inference are the laws of physics ; thus the law that light travels in straight lines enables us to infer the location of distant objects ; and so on. In fact a law of physics can be used either way, to predict an effect from a cause or to infer a cause (*i.e.* a physical object embodying certain properties) from an observed effect.

In the universe of inferences past present and future appear simultaneously and it requires scientific analysis to sort them out. By a certain rule of inference, *viz.* the law of gravitation, we infer the present or past existence of a dark companion to a star ; by an application of the same rule of inference we infer the existence on August 11, 1999, of a configuration of the sun, earth and moon, which corresponds to a total eclipse of the sun. The shadow of the moon on Cornwall in 1999 is already in the universe of inference. It will not change its status when the year 1999 arrives and the eclipse is observed ; we shall merely substitute one method of

inferring the shadow for another. The shadow will always be an inference. I am speaking of the object or condition in the external world which is called a shadow; our perception of darkness is not the physical shadow, but is one of the possible clues from which its existence can be inferred.

Of particular importance to the problem of determinism are our inferences about the past. Strictly speaking our direct inferences from sight, sound, touch, all relate to a time slightly antecedent; but often the lag is more considerable. Suppose that we wish to discover the constitution of a certain salt. We put it in a test-tube, apply certain reagents, and ultimately reach the conclusion that it *was* silver nitrate. It is no longer silver nitrate after our treatment of it. This is an example of retrospective inference: the property which we infer is not that of "being *X*" but of "having been *X*".

We noted at the outset that in considering determinism the alleged causes must be challenged to produce their birth certificates so that we may know whether they really were pre-existing. Retrospective inference is particularly dangerous in this connection because it involves antedating a certificate. The experiment above-mentioned certifies the chemical constitution of a substance, but the date we write on the certificate is earlier than the date of the experiment. The antedating is often quite legitimate; but that makes the practice all the more dangerous, it lulls us into a feeling of security.

Retrospective Characters.

To show how retrospective inference might be abused, suppose that there were no way of learning the chemical constitution of a substance without destroying it. By hypothesis a chemist would never know until after his experiment what substance he had been handling, so that the result of every experiment he performed would be entirely unforeseen. Must he then admit that the laws of chemistry are chaotic? A man of resource would override such a trifling obstacle. If he were discreet enough never to say beforehand what his experiment was going to demonstrate, he might give edifying lectures on the uniformity of nature. He puts a lighted match in a cylinder of gas and the gas burns. "There you see that hydrogen is inflammable." Or the match goes out. "That proves that nitrogen does not support combustion." Or it burns more brightly. "Evidently oxygen feeds combustion." "How do you know it was oxygen?" "By retrospective inference from the fact that the match burned more brightly." And so the experimenter passes from cylinder to cylinder; the match sometimes behaves one way and sometimes another, thereby beautifully demonstrating the uniformity of nature and the determinism of chemical law! It would be unkind to ask how the match must behave in order to indicate indeterminism.

If by retrospective inference we infer characters at an earlier date and then say that those characters invariably produce at a future date the manifestation from which we inferred them, we are working

in a circle. The connection is not causation but definition, and we are not prophets but tautologists. We must not mix up the genuine achievements of scientific prediction with this kind of charlatany, nor the observed uniformities of nature with those so easily invented by our imaginary lecturer. It is easily seen that to avoid vicious circles we must abolish purely retrospective characteristics—those which are never found as existing but always as having existed. If they do not manifest themselves until the moment that they cease to exist, they can never be used for prediction except by those who prophesy after the event.

Chemical constitution is not a retrospective character though it is often inferred retrospectively. The fact that silver nitrate can be bought and sold shows that there is a property of *being* silver nitrate as well as of *having been* silver nitrate. Apart from special methods of determining the constitution or properties of a substance without destroying it, there is one general method widely applicable. We divide the specimen into two parts, analyse one part (destroying it if necessary) and show that its constitution *has been* X ; then it is usually a fair inference that the constitution of the other part *is* X . It is sometimes argued that in this way a character inferable retrospectively must always be also inferable contemporaneously; if that were true it would remove all danger of using retrospective inference to invent fictitious characters as causes of the events observed. Actually the danger arises just at the point where the method of sampling breaks down, viz. when we are concerned with characteristics supposed to distinguish one individual atom from another atom of the same substance; for the individual atom cannot be divided into two samples, one to analyse and one to preserve. Let us take an example.

It is known that potassium consists of two kinds of atoms, one kind being radioactive and the other inert. Let us call the two kinds K_a and K_b . If we observe that a particular atom bursts in the radioactive manner we shall infer that it was a K_a atom. Can we say that the explosion was predetermined by the fact that it was a K_a and not a K_b atom? On the information stated there is no justification at all; K_a is merely an antedated label which we attach to the atom when we see that it has burst. We can always do that however undetermined the event may be which occasions the label. Actually, however, there is more information which shows that the burst is not undetermined. Potassium is found to consist of two isotopes of atomic weights 39 and 41; and it is believed that 41 is the radioactive kind, 39 being inert. It is possible to separate the two isotopes and to pick out atoms known to be K_{41} . Thus K_{41} is a contemporaneous character and can legitimately predetermine the subsequent radioactive outburst; it replaces K_a which was a retrospective character.

So much for the fact of outburst; now consider the time of outburst. Nothing is known as to the time when a particular K_{41} atom will burst except that it will probably be within the next thousand million years. If, however, we observe that it bursts at a

time t we can ascribe to the atom the retrospective character K_t , meaning that it had (all along) the property that it was going to burst at time t . Now according to modern physics the character K_t is not manifested in any way—is not even represented in our mathematical description of the atom—until the time t when the burst occurs and the character K_t having finished its job disappears. In these circumstances K_t is not a predetermining cause. Our retrospective labels and characters add nothing to the plain observational fact that the burst occurred without warning at the moment t ; they are merely devices for ringing a change on the tenses.

The time of break up of a radioactive atom is an example of extreme indeterminism; but it must be understood that according to current theory all future events are indeterminate in greater or lesser degree, and differ only in the margin of uncertainty. When the uncertainty is below our limits of measurement the event is looked upon as practically determinate; determinacy in this sense is relative to the refinement of our measurements. A being accustomed to time on the cosmic scale, who was not particular to a few hundred million years or so, might regard the time of break up of the radioactive atom as practically determinate. There is one unified system of secondary law throughout physics and a continuous gradation from phenomena predictable with overwhelming probability to phenomena which are altogether indeterminate.

The statement that all phenomena have some degree of indeterminacy will probably be criticised as too sweeping. I will consider just one example. I have said that a K_{39} atom is not radioactive. Then (it will be said) we can at least state one predetermining fact about its future; we can predict without any indeterminacy that it will not break up as a K_{41} atom would do. The answer of modern physics is that strictly speaking there is no such thing as a K_{39} atom, but only an atom which has a high probability of being K_{39} . Such an atom should contain 39 protons within a small nucleus; but the proton in modern physics has a very important peculiarity, viz. it never is anywhere quite definitely though it may have a greater probability of being in one place rather than another. Thus we can never get beyond a high probability of 39 protons being collected together. It is impossible to trap modern physics into predicting anything with complete determinacy, because it deals with probabilities from the outset.

It has seemed necessary for clearness to give an example of an event believed to be widely indeterminate; but you must not suppose that I have brought forward the phenomenon of radioactivity as evidence for indeterminism. There is a widespread idea that physicists, having spent a few years investigating certain phenomena and being baffled to discover a cause, have jumped to the conclusion that there is no cause. That is not the way in which the idea of indeterminacy came into physics. I have tried to explain how it originated in the earlier part of this address.

Criticism of Indeterminism.

In saying that there is no contemporaneous characteristic of the radioactive atom determining the date at which it is going to break up, we mean that in the picture of the atom as drawn in present-day physics no such characteristic appears ; the atom which will break up in 1960 and the atom which will break up in the year 150000 are drawn precisely alike. But, you will say, surely that only means that the characteristic is one which physics has not yet discovered ; in due time it will be found and inserted in the picture either of the atom or of its environment. If such indeterminacy were exceptional that would be the natural conclusion and we should have no objection to accepting such an explanation as a likely way out of a difficulty. But the radioactive atom was not brought forward as a difficulty ; it was brought forward as a favourable illustration of that which applies in greater or lesser degree to all kinds of phenomena. There is a difference between explaining away an exception and explaining away a rule.

The persistent critic continues, " You are evading the point. I contend that there are characteristics unknown to you which completely predetermine not only the time of break up of the radioactive atom but all physical phenomena. How do you know there are not ? You are not omniscient ". It is at this point I want to shout and wake my critic. So I will tell you a story.

About the year 2000, the famous archaeologist Professor Lambda discovered an ancient Greek inscription which recorded that a foreign prince, whose name was given as Κανδείκλης, came with his followers into Greece and established his tribe there. The professor anxious to identify the prince, after exhausting other sources of information, began to look through the letters C and K in the Encyclopaedia Athenica. His attention was attracted by an article on Canticles who it appeared was the son of Solomon. Clearly that was the required identification ; no one could doubt that Κανδείκλης was the Jewish prince Canticles. His theory attained great notoriety. At that time the Great Powers of Greece and Palestine were concluding an Entente and the Greek Prime Minister in an eloquent peroration made touching reference to the newly discovered historical ties of kinship between the two nations. Some time later Professor Lambda happened to refer to the article again and discovered an unfortunate mistake ; he had misread " Son of Solomon " for " Song of Solomon ". The correction was published widely, and it might have been supposed that the Canticles theory would die a natural death. But no ; Greeks and Palestinians continued to believe in their kinship, and the Greek Minister continued to make perorations. Professor Lambda one day ventured to remonstrate with him. The Minister turned on him severely, " How do you know that Solomon had not a son called Canticles ? You are not omniscient ". The Professor, having reflected on the rather extensive character of Solomon's matrimonial adventures, wisely made no reply.

The curious thing is that the determinist who takes this line is under the illusion that he is adopting a more modest attitude in regard to our scientific knowledge than the indeterminist. The indeterminist is accused of claiming omniscience. I will not make quite the same countercharge against the determinist ; but surely it is only the man who thinks himself *nearly* omniscient who would have the audacity to start enumerating all the things which (it occurs to him) might exist without his knowing it. I am so far from omniscient that my list would contain innumerable entries. If it is any satisfaction to the critic, my list does include deterministic characters—along with Martian irrigation works, ectoplasm, etc.—as things which might exist unknown to me.

It must be realised that determinism is a positive assertion about the behaviour of the universe. It is not sufficient for the determinist to claim that there is no fatal objection to his assertion ; he must produce some reason for making it. I do not say he must prove it, for in science we are ready to believe things on evidence falling short of strict proof. If no reason for asserting it can be given, it collapses as an idle speculation. It is astonishing that even scientific writers on determinism advocate it without thinking it necessary to say anything in its favour, merely pointing out that the new physical theories do not actually disprove determinism. If that really represents the status of determinism no reputable scientific journal would waste space over it. Conjectures put forward on slender evidence are the curse of science ; a conjecture for which there is no evidence at all is an outrage. So far as the physical universe is concerned determinism appears to explain nothing ; for in the modern books which go farthest into the theory of the phenomena no use is made of it.

Indeterminism is not a positive assertion. I am an indeterminist in the same way that I am an anti-moon-is-made-of-green-cheese-ist. That does not mean that I especially identify myself with the doctrine that the moon is not made of green cheese. Whether or not this lunar theory can be reconciled with modern astronomy is scarcely worth inquiring ; the main point is that green-cheeseism like determinism is a conjecture that we have no reason for entertaining. Undisprovable hypotheses of that kind can be invented *ad lib.*

Principle of Uncertainty.

The mathematical treatment of an indeterminate universe does not differ much in form from the older treatment designed for a determinate universe. The equations of wave-mechanics used in the new theory are not different in principle from those of hydrodynamics. The fact is that, since an algebraic symbol can be used to represent either a known or an unknown quantity, we can symbolise a definitely predetermined future or an unknown future in the same way. The difference is that whereas in the older formulae every symbol was theoretically determinable by observation, in the present theory there occur symbols whose values are not assignable by observation.

Hence if we use the equations to predict say the future velocity of an electron the result will be an expression containing besides known symbols a number of undeterminable symbols. The latter make the prediction indeterminate. I am not here trying to prove or explain the indeterminacy of the future ; I am only stating how we adapt our mathematical technique to deal with an indeterminate future. The indeterminate symbols can often (or perhaps always) be expressed as unknown phase-angles. When a large number of phase-angles are involved we may assume in averaging that they are uniformly distributed from 0° to 360° , and so obtain predictions which could only fail if there has been an unlikely coincidence of phase-angles. That is the secret of all our successful prophecies ; the unknowns are eliminated not by determinate equations but by averaging.

There is a very remarkable relation between the determined and the undetermined symbols which is known as Heisenberg's Principle of Uncertainty. The symbols are paired together, every determined symbol having an undetermined symbol as partner. I think that this regularity makes it clear that the occurrence of undetermined symbols in the mathematical theory is not a blemish ; it gives a special kind of symmetry to the whole picture. The theoretical limitation on our power of predicting the future is seen to be systematic, and it cannot be confused with other casual limitations due to our lack of skill.

Let us consider an isolated system. It is part of a universe of inference, and all that can be embodied in it must be capable of being inferred from the influence which it broadcasts over its surroundings. Whenever we state the properties of a body in terms of physical quantities we are imparting knowledge as to the response of various external indicators to its presence and nothing more. A knowledge of the response of all kinds of objects would determine completely its relation to its environment, leaving only its un-get-at-able inner nature which is outside the scope of physics. Thus if the system is really isolated so that it has no interaction with its surroundings, it has no properties belonging to physics but only an inner nature which is beyond physics. So we must modify the conditions a little. Let it for a moment have some interaction with the world exterior to it ; the interaction starts a train of influences which may reach an observer ; he can from this one signal draw an inference about the system, *i.e.* fix the value of one of the symbols describing the system or fix one equation for determining their values. To determine more symbols there must be further interactions, one for each new value fixed. It might seem that in time we could fix all the symbols in this way so that there would be no undetermined symbols in the description of the system. But it must be remembered that the interaction which disturbs the external world by a signal also reacts on the system. There is thus a double consequence ; the interaction starts a signal through the external world informing us that the value of a certain symbol p in the system is p_1 , and at the same time it alters to an indeterminate extent the

value of another symbol q in the system. If we had learned from former signals that the value of q was q_1 , our knowledge will cease to apply, and we must start again to find the new value of q . Presently there may be another interaction which tells us that q is now q_2 ; but the same interaction knocks out the value p_1 and we no longer know p . It is of the utmost importance for prediction that a paired symbol and not the inferred symbol is upset by the interaction. If the signal taught us that at the moment of interaction p was p_1 , but that p had been upset by the interaction and the value no longer held good, we should never have anything but retrospective knowledge—like the chemistry lecturer whom I described. Actually we can have contemporaneous knowledge of the values of half the symbols, but never more than half. We are like the comedian picking up parcels; each time he picks up one he drops another.

There are various possible transformations of the symbols and the condition can be expressed in another way. Instead of two paired symbols, the one wholly known and the other wholly unknown, we can take two symbols each of which is known with some uncertainty; then the rule is that the product of the two uncertainties is fixed. Any interaction which reduces the uncertainty of determination of one increases the uncertainty of the other. For example, the position and velocity of an electron are paired in this way. We can fix the position with a probable error of .001 millimetres and the velocity with a probable error of about 1 km. per sec.; or we can fix the position to .0001 millimetres and the velocity to 10 km. per sec.; and so on. We divide the uncertainty how we like but we cannot get rid of it. If current theory is right, this is not a question of lack of skill or a perverse delight of Nature in tantalising us, for the uncertainty is actually embodied in the theoretical picture of the electron; if we describe something as having exact position and velocity we cannot be describing an electron, just as (according to Russell) if we describe a person who knows what he is talking about and whether what he is saying is true we cannot be describing a pure mathematician.

If we divide the uncertainty in position and velocity at time t_1 in the most favourable way we find that the predicted position of the electron 1 second later at time t_2 is uncertain to about 5 centimetres. That represents the extent to which the future position is not predetermined by anything existing 1 second earlier. If the position at time t_2 always remained uncertain to this extent there would be no failure of determinism.* But when the second has elapsed we can measure the position of the electron to .001 millimetres or even more closely, as already stated. This accurate position is not predetermined; we have to wait until the time arrives and then measure it. It may be recalled that the new knowledge is acquired at a price. Along with our rough knowledge of position (to 5 cms.)

* For the thing we failed to predict (exact position at time t_2) would be meaningless.

we had a fair knowledge of the velocity ; but when we acquire more accurate knowledge of the position the velocity goes back into extreme uncertainty.

We might spend a long while admiring the detailed working of this cunning arrangement by which we are prevented from finding out more than we ought to know. But I do not think you should look on these as Nature's devices to prevent us from seeing too far into the future. They are the devices of the mathematician who has to protect himself from making impossible predictions. It commonly happens that when we ask silly questions, mathematical theory does not directly refuse to answer but gives a non-committal answer like $0/0$ out of which we cannot wring any meaning. Similarly when we ask where the electron will be to-morrow, the mathematical theory does not give the straightforward answer "It is impossible to say because it is not yet decided"—because that is beyond the resources of an algebraic vocabulary. It gives us an ordinary formula of x 's and y 's, but makes sure that we cannot possibly find out what the formula means—until to-morrow.

Mind and Indeterminism.

I have, perhaps fortunately, left myself no time to discuss the effect of indeterminacy in the physical universe on our general outlook. I will content myself with stating in summary form the points which seem to arise.

(1) If the whole physical universe is deterministic, mental decisions (or at least *effective* mental decisions) must also be predetermined. For if it is predetermined in the physical world, to which your body belongs, that there will be a pipe between your lips on January 1, the result of your mental struggle on December 31 as to whether you will give up smoking in the New Year is evidently predetermined. The new physics thus opens the door to indeterminacy of mental phenomena, whereas the old deterministic physics bolted and barred it completely.

(2) The door is opened slightly, but apparently the opening is not wide enough. For according to analogy with inorganic physical systems we should expect the indeterminacy of human movements to be quantitatively insignificant. In some way we must transfer to human movements the wide indeterminacy characteristic of atoms instead of the almost negligible indeterminacy manifested by inorganic systems of comparable scale. I think this difficulty is not insuperable, but it must not be underrated.

(3) Although we may be uncertain as to the intermediate steps we can scarcely doubt what is the final answer. If the atom has indeterminacy, surely the human mind will have an equal indeterminacy ; for we can scarcely accept a theory which makes out the mind to be more mechanistic than the atom.

(4) Is the human will really more free if its decisions are swayed by new factors born from moment to moment than if they are the outcome solely of heredity, training and other predetermining causes ?

On such questions as these we have nothing new to say. Argument will no doubt continue "about it and about". But it seems to me that there is a far more important aspect of indeterminacy. It makes it possible that the mind is not utterly deceived as to the mode in which its decisions are reached. On the deterministic theory of the physical world my hand in writing this address is guided in a pre-determined course according to the equations of mathematical physics; my mind is unessential—a busybody who invents an irrelevant story about a scientific argument as an explanation of what my hand is doing—an explanation which can only be described as a downright lie. If it is true that the mind is so utterly deceived in the story it weaves round our human actions, I do not see where we are to obtain our confidence in the story it tells of the physical universe.

Physics is becoming difficult to understand. First relativity theory, then quantum theory, then wave mechanics have transformed the universe making it seem ever more fantastic to our minds. Perhaps the end is not yet. But there is another side to this transformation. Naïve realism, materialism, the mechanistic hypothesis were simple; but I think that it was only by closing our eyes to the essential nature of experience, relating as it does to the reactions of a conscious being, that they could be made to seem credible. These revolutions of scientific thought are clearing up the deeper contradictions between life and theoretical knowledge, and the latest phase with its release from determinism marks a great step onwards. I will even venture to say that in the present theory of the physical universe we have at last reached something which a reasonable man might almost believe.

A. S. EDDINGTON.

GLEANINGS FAR AND NEAR.

847. On sait . . . depuis long-temps, par plusieurs propositions importantes, et particulièrement par le théorème du père Guldin . . . l'avantage que peut tirer la géométrie elle-même de la considération des centres de gravité; mais cette marche n'est point naturelle. C'est donc une véritable lacune dans les traités de géométrie, que l'omission de ses propriétés fondamentales; et je pense que ce seraient faire une chose utile que de rétablir à cet égard l'ordre naturel des idées, en développant dans la géométrie même les propriétés du centre de moyennes distances, qui ne devient réellement *centre de gravité* qu'en mécanique. Les ressources de la géométrie élémentaire, qui est la plus usuelle, seraient beaucoup plus considérables, et une multitude de propositions très-utiles dans la pratique, et que cette géométrie ne saurait atteindre que fort difficilement, se démontreroient avec une extrême simplicité.

Il me semble même que la géométrie ne devrait point se borner là, et qu'elle pourroit embrasser les mouvements, qui ne résultent pas de l'action et de la réaction des corps les uns sur les autres; car la mécanique n'est pas proprement la science des mouvements, mais la science de la communication des mouvements.—L. N. M. Carnot, *Géométrie de Position* (1803), 336.

THE REPORT OF THE ASSOCIATION OF UNIVERSITY TEACHERS ON ENTRANCE TESTS AND INITIAL DEGREES.*

MR. J. T. COMBRIDGE, in opening the discussion, said: My first duty is to give you a brief summary of the *Report on Entrance Tests and Initial Degrees*, adopted in June 1930 by the Council of the Association of University Teachers. My second duty is to offer such comments on it as may provoke a heated discussion among those who might otherwise be disposed to agree in approving or condemning it. For the benefit of those who have a copy of the *Report*, I would say that the mysterious reference at the foot of p. 4, "See pp. 4-10", in reference to the English test, is due to the fact that the *Report* is an offprint from a certain number of the *Universities Review*, and the test referred to appears on pp. 4-10 of that number.

The *Report* deals separately with Entrance Tests and Initial Degrees; it is mainly with the former that we shall be concerned this morning. But it finds that both are productive of the same disease of premature admission and over-specialisation, and that both may be used to provide a remedy. It points out that the qualifications for matriculation have been much modified in recent years, and that consequently

- (1) matriculation is no longer a guarantee of fitness for university work;
- (2) it is so linked up with School Certificates that the requirements of the universities dominate the work of large numbers who will never enter them;
- (3) students frequently come to the universities having taken the Higher School Certificate and so claim admission to Honours classes. Specialisation is thus not only spread among students unsuited for it, but extends down to the upper forms of schools and is reflected in the Higher School Certificate syllabuses;
- (4) the pass degree, intended to be the mark of a broad education, has in consequence been severely depreciated.

The *Report* makes suggestions for remedying this state of affairs. Among other things, it suggests certification by the staff, and utilisation by the universities of the Certificate qualification together with a further test guaranteeing a certain standard of general education.

The section on Initial Degrees recommends General and Special Degree courses of three years, having their first-year syllabuses similar.

Before commenting on the *Report*, I would like to point out that, on the one hand, this is very much the concern of university teachers; they spend much of their time teaching the students sent from the

* A discussion of the *Report* at the Annual Meeting, 4th January, 1932.

schools ; many of these students go back to the schools as teachers ; and the schools do in fact follow, whether reluctantly or not, the pace set by the universities ; on the other hand, university teachers, even more than the universities—please don't confuse the two—are handicapped by a lack of means of working out and enforcing a common university policy.

Coming to the disease with which the *Report* deals, we shall find, I think, that we are all agreed as to the symptoms, and that there is little disagreement over the diagnosis. From the point of view of provocative discussion this is unfortunate. We all think at once of the various Matriculation and School Certificate examinations, and it is quite easy to give one's self up to abusing them. I am not going to encourage running down the men who set the papers. I have acted as assistant examiner in several of these examinations, and I know the difficulty of producing a question of the right standard which has not been set before, which occurs in no text-book, and for which no candidate could conceivably have been crammed. There is the difficulty of devising not only a set of questions (not of "scholarship" type), but also a scheme of marking, which firmly and decisively eliminates the candidate who "gets sums right" in a way that shows complete ignorance of the principles of the subject.

Expecting, then, fairly general agreement on the evils arising from the too complete interlocking of School Certificate and Matriculation examinations, and from too facile admission to specialised degree courses, I think my best plan is, first, to say a little about the way in which those evils affect our Intermediate classes, where we have students who have to pass, at the end of their first year at the university, an examination in four subjects before beginning their degree course, which may involve three or only two subjects.

At one end of such classes we have students who have just scraped through in mathematics in some form of matriculation examination. They can just "do sums", and often there is little compensating knowledge in other subjects. (One of them once performed a long piece of work but failed to carry out the final computation. I wrote on her paper : "Tantus labor non sit cassus. Use logs." When I was going round in the exercise class after her paper had been returned she asked me about the Latin advice, and her neighbour said : "Does it mean 'Use logs' ?" I replied, "No, it's the inscription over the door of the College chapel, which faces you as you go out of this class-room".) Once through the gate at the end of the first year—and our Intermediate examination is a gate and not a fence—they will do no more mathematics, and if we do not do our best to push them through this gate we may be ruining the career of a highly promising botanist or French Honours student. Mathematics, as a subject in the faculties of both Arts and Science, is too often the fourth subject which must be found by hook or by crook. "But why did you take mathematics for Inter. ?" "Well, what else could I take ?"

At the other end of these classes are students who have had two

years longer at school and have just failed a Higher Schools examination, who listen disdainfully to all lectures because the sounds are familiar, and who need to be kept supplied with ingenious problems to make them realise that although they have covered the syllabus there are still some portions of it which they have not understood.

In the stress of instilling the right amount of humility into these, and doing our duty to the others, we are apt to overlook the student who really deserves our attention : the type who can jog along alone, who has the makings of a good General or Special Honours student and only needs developing. Such a one is neglected, or spoon-fed with the weaker ones, and relapses into a state of complete incapacity for taking proper advantage of a degree course.

It is no solution to ignore the weaklings. They are the victims of a system which, in effect, says that a pupil of sixteen of average learning is sufficiently educated to take advantage of university methods of instruction.

The classes of the degree courses are thus composed largely of untrained but well-crammed survivors of the Intermediate exam., and untrained but better crammed arrivals from the schools. By "untrained" I mean "not trained to read for themselves"; by "crammed" I indicate an aversion from everything not in the syllabus for their degree subjects. (I am bound to add that under the present system the universities are sinking to the state from which the schools are rising—that of cramming institutions. Before the schools have learnt the art of giving their pupils not only knowledge but the power to acquire it, the universities will have lost the art completely.) Thus the uneducated ones produced by premature admission to the university, and the uneducated ones who are victims of premature specialisation, go on side by side to be lectured into a degree, and to intensify the commercial world's distaste for university products or to maintain the barrier between the teaching profession and their fellow men and women.

I have based my remarks on my own experience, but if any think I am making sweeping generalisations I refer them to Lord Eustace Percy's *Education at the Cross Roads*.* Do not imagine that the state of affairs I have outlined has undisputed dominance ; but it is a state which demands and receives incessant warfare, and to which we are in danger of becoming more and more submissive by sheer custom and inertia. It may be well to pause here and examine the two objects of our animosity. That premature admission to a university is an evil, few will deny ; it may be remedied either by improving the general education before admission or by increasing specialisation. The latter therefore should receive our closest scrutiny, and I think we shall agree that while specialisation in something is desirable for most, excessive specialisation, of which premature specialisation is the forerunner, is an evil. We may, of course, adopt the outlook of Mr. H. G. Wells's inhabitants of the Moon, who had brought specialisation to the pitch of perfection,

but in our present imperfect world, apart from the uneconomic and social advantages of a general education, it aggravates unemployment. A student begins to be shaped for a particular kind of niche when there are plenty of empty niches, but the same shaping process is commenced on others regardless of the fact that by the time it is finished all these niches will be filled. I quite believe that we are turning out hundreds of graduates who are fit for nothing (economically) but to be absorbed into the system which produces them, and which must ultimately decay from its own inbreeding. The moment a university ceases to produce men and women who, because of the extent of their general education, are better specialists than they would otherwise have been, and who, moreover, can leave their special branch and develop another if required, it has not only ceased to perform its proper function but has become a potential source of economic danger. If I may quote the concluding words of Lord Eustace Percy's book : " Employers who are asked by ' educationalists ' to say what they want from the schools will hardly think it necessary to say that they want educated men. They assume that, as the ordinary Englishman traditionally assumes it. The danger, the really desperate danger, at the present day is that this is just the one thing that schools and colleges and universities may fail to supply."

One remedy proposed by the *Report* is that the close and direct connection between the School Certificate and the qualification for Matriculation should be abolished ; that entrants to the universities should be about eighteen years of age or more, and that there should be a test passed by candidates for matriculation prior to entry, at that age, which should involve four subjects (not necessarily all at the same level) ; and that the use of English should be a part of this test. To this is added a certificate from the school (not from the headmaster alone), giving guidance as to the entrant's powers and interests. This remedy I shall leave others to expound and discuss.

Another remedy suggested is the institution of " a General course leading to a General degree, and a more specialised course, leading to a Special degree ". This we already have in London, but the novel feature is the suggestion : " both courses should be planned to provide three years of full-time study to students entering with the revised entrance qualification . . . both the General and the Special courses should be built on approximately the same first-year scheme in the university, so that it may be possible to transfer students from either side to the other at or before the end of their first year ". The latter provision would be most welcome, but it is not our main concern now. Taking the two remedies together, it seems to me that their effect is to clear all our present Intermediate course work out of the university into the schools, and to leave us that year in which to educate students, who at present start on a two-year degree course after reaching the Intermediate standard in the state of mind I described earlier—to educate them, I say, in the right use of a library, in the right use of a lecture, and in the right

use of a text-book. I should welcome this change, not because I object to teaching the elements of a subject, but because there is something even more important which a student should begin to learn—or rather discover—in the first year at a university. Our present system does not allow—as I believe it should—a clear year for enabling the entrant to the university to become acclimatised to the change and to the new mentality which should come about that age.

There is one aspect of the problem not—ironically enough—mentioned in the *Report*. Promotion in a university career depends almost entirely on the amount of published work. One effect of this *must* be that university teachers pay an undue regard to specialisation; they cannot afford to assist a research student on any line of knowledge but their own narrow one, and, moreover, excessive research must taint their teaching. Further, promotion in a scholastic career—that is, the acquisition of a headship—depends largely on impressing superstitious bodies of governors and local authorities with a higher degree. One effect of this *must* be to create a class of post-graduate research workers, who would be much happier and much better spending their time on the problems of pedagogy, for which no degree but the answer of a good conscience is offered. With university and school teachers affected in this way, it is no wonder that any pupil who takes an interest in his or her future gets the idea that it pays to specialise, to specialise early and to specialise always, and that what does *not* pay is, to reserve to one's self time to think.

I have dealt with the subject in very condensed form, and have said nothing whatever as to the attitude of the outside world to Matriculation and the School Certificate. A school mistress told me only last week that it is necessary to cater for Matric. because that is what employers want. This morning I read that what employers want is the School Certificate Examination—I only hope it is—and not Matriculation.

I have been fortunate in securing Miss Stimson of Whitelands Training College, and Mr. Heath of St. Paul's School (who a few years ago initiated a junior edition of the London Mathematical Society among London schoolboys) to deal with some of the topics of the *Report* with which I am not fitted to deal. I hope it will be realised that I have had to condense as far as possible and that I have left out a great deal which I should say if I had more time and if I were not being reported.

Miss C. Stimson (Whitelands Training College) then said: I have been asked to put forward points on this subject which concern two-year training colleges for women; that is, the colleges which supply certificated women teachers to elementary schools. When I consented to do this, I did not remember that the majority of this meeting would be men. What I say may apply to men's colleges. There are, no doubt, those among you to whom these colleges are not foreign territory, and we may hear something from you in the discussion.

The training colleges have recently become linked to the universities, since the final Teacher's Certificate Examination which terminates the two-year course is now a university concern.

The training colleges receive their students from the secondary schools, generally speaking at the end of a Higher Certificate course. Thus they are concerned in any reforms of the school examinations. The minimum qualification for admission to a training college is a pass in School Certificate—not Matriculation. Candidates then apply to enter at eighteen. In selecting students the college may consider the school report, the way in which post-School Certificate years have been spent, and a personal interview. There is no entrance examination.

At Whitelands of students of the last two years about 60 per cent. have matriculated, and about 33 per cent. have passed a Higher Certificate examination. These qualifications are some guide for admission. They may also give the staff some idea of attainment. Actually we take very little, if any, notice of them afterwards, chiefly because we are too concerned with noticing the effect of the system of which they are the result.

The students who have taken Higher Certificates are certainly over-crammed and have over-specialised. Besides this, the general time-table in the school is often planned for the examination pupils. Non-examination pupils have to fit in where they can. The result is sometimes an ill-balanced and planless course for them.

We receive what I will term B to C people from the schools. We rarely get an A student. They come up to college with an honest desire to make the best use of their course. They interpret this desire into an earnest endeavour to believe everything we tell them. We are an oracle whose superstitions are to be treated with awe. From this we see they have as yet no sense of values nor power to criticise.

We find also that they cannot express themselves adequately in either spoken or written English, and, although their paper qualifications say to the contrary, their arithmetic is much below School Certificate standard. Most of them do not know how to use books, but they are not unintelligent nor uninformed people. The trouble is that they have been over-taught. They do not yet know how to learn. This we can only put down to the system's passionate belief in examinations and the cramming and over-specialisation that goes with this.

If the suggestions in the *Report* were carried out, the potential training college students would be freed from one examination. The training colleges would then find that instead of the Matriculation and Higher Certificate qualification for some entrants, all entrants would possess a certificate granted by the school. These certificates, even more than the Higher Certificates, would lack a common standard, and the training colleges would learn to be interested in them, but would not attach too much importance to them.

Having freed these non-university students from the examination,

I sincerely trust that the schools would organise courses for all their non-university post-School Certificate people, quite independent of the courses for university entrance. I also hope that attention would not weigh more heavily on the one side than on the other. But I sometimes fear the worst. A course with an examination at the end seems always more important to the schools than one without. Also the staffs of schools are themselves a product of universities. Thus the non-university side may be at a disadvantage, except there be individuals on the staff who have a peculiar interest in this side, and these will, I fear, be the exception rather than the rule.

This side will, too, I think, consist largely of the B and C people. I should suggest that they are not rushed through School Certificate too early. Some of them could quite well postpone it until, say, seventeen. We should then not have in training colleges students who have obviously been crammed for the first school examination. For the rest of this school course we might do worse than consider schemes similar to the tutorial classes of the W.E.A.

The definite cut between the two sides might also prevent, to some extent, pupils trying two courses at once. For instance, university entrance as a first try, and if a failure, then a resort to a two-year college. We sympathise with their desire for the best possible, but if they were restricted to a more single aim their education in the post-School Certificate years would be of more assured success.

During my experience in a training college I have come to think that a clear understanding of the methods and teaching of at least one other subject—and if possible more—does much towards improving the teaching of one's own special subject. This is very difficult if one has taken a highly specialised degree. It would be interesting to have the opinion of, say, a university training department on whether a General Honours degree is probably a better teaching qualification than a Special Honours.

In conclusion, I think the *Report* leaves the last school years still in danger of being dominated by university entrance and, therefore, preserving the cram atmosphere. I should think those years better spent in learning how to read, how to write and how to think; and while the preparation for entrance is the work of the schools, I fear they will tend to overlook these needs because their view will be obscured by the examination pass list.

Mr. A. C. Heath : We most of us, at some time or other, have come across the evil effects of the present School Certificate examination. We all know the boy who has passed with four and five credits, staying on at school, taking the examination again and again, in vain pursuit of that one essential credit which will make his certificate a "Matriculation" one, and so ensure his possession of a junior clerkship in his uncle's emporium. It is hardly too much to say that the School Certificate in its present form has quite definitely lowered the standard of knowledge in our schools. In the dim and distant past, before all schools took the School Certificate, I had

the pleasure to teach the top grade of a block of mathematical sets all on the classical side. The usual School Certificate ground was covered, but there was plenty of time to stray into interesting little byways. An interest in mathematics was aroused, and one was able to show to some of the boys the hidden beauties of our subject. And incidentally if you set them an examination paper the form would average 75 per cent. Now all is changed. The block of sets take the School Certificate. The bright able boys pass early in a lower grade, and are anxious "to get the subject out of the way". They go off to their classical work. Those who survive to the top grade, "crocks of ages left for me", average about 35 per cent. on a paper. As a colleague remarked: "We used to have three terms a year to teach in; now we have only one—the other two are needed to cram for the examination".

It is, therefore, with joy that we welcome the main propositions in the *Report*.

To my mind, boys who leave us for the universities—the only boys we are concerned with at the moment—are of two types. There is the boy with the definite bent for some special subject who nowadays specialises early. The university teachers in their report tend to deplore this specialisation. I am not sure that they are right. The older universities—by their scholarships—cater well for the gifted boy; and the talented boy who has spent two years working for one of these prizes is—even if unsuccessful—by no means uneducated. Let me describe the programme of a form with which I am acquainted. The boy with a gift for mathematics makes that his principal study and learns to work hard, and by himself, without set home-works and the like. He also studies a kindred subject, probably physics. He takes at least one foreign language, one course in English where he learns to talk and write intelligently on serious topics of the day, and another where he learns sufficient about music, painting, architecture and literature to enable him eventually to obtain real enjoyment from his own private interest in them. Such a boy, after two years, is, to my mind, by no means ill-educated. He has enough knowledge to be able to enjoy the beautiful things of life, and he has learned one subject well; he has learned to work, and to work by himself. The older universities by their scholarships (including their excellent general paper) cater for this boy. The newer universities, if they want him, would do well to copy.

The other type of boy who goes on to the universities—and he usually goes to one of the newer universities—is the boy who does not show an especial bent at an early age. It is here that the Matriculation exam. of to-day so singularly fails to bring to the university the student who will profit by the facilities it offers. Well do I remember, as a university teacher, the labour Mr. Combridge has so vividly described, necessary to get the "just matriculated" one through the "intermediate gate". The university teachers in this *Report* put forward a scheme with which we shall all be more or less in agreement. And, after all, it is for them to

state what they require of entrants "at about 18", and for us teachers to see that those requirements are fulfilled. But before they make this much-to-be-desired change and bring out their new Matriculation syllabus, let me give them two warnings. Anyone who has read the *Report* and has followed out what the mysterious letters " pp. 4-10 " mean, will have found that they have left their examination on the right use of English to one whom one takes to be a specialist in English literature. I think that is a great mistake. I well remember a conversation with an official in the education department of one of our newer universities. " I get graduates from all faculties ", he said, " and they all write me essays. The worst are the Honours English men ; the best the scientists—they say what they have to say and then stop ".

Secondly, I see no reason in the suggestion in the *Report* that the universities should have any say, however slight, in the standard of general education at the present School Certificate age. Their concern should be only whether a boy is fit to profit by university instruction "at about 18". To-day we all know the folly of assuming that a boy who gets a credit in some subject in his School Certificate and then drops it for two years can make use of it at the university ; and the converse is just as true. What should take the place of the present School Certificate for boys leaving school and not going up to the university is an enthralling subject outside the scope of this discussion, but I do hope it will be entirely free from anything to do with Matriculation. If we let them have this evidence of satisfactory education, they will go on extending it, and then we shall have them insisting that all intending students shall have been submitted to the Allenbury system of infant feeding !

Apart from these two criticisms, I am sure we can all welcome the tendency shown in the *Report*, and congratulate the university teachers on a step in the right direction.

Mr. Hope-Jones (Eton) : I should like to ask three questions. In the middle of p. 4 of the *Report* four subjects are recommended. Are Pure and Applied Mathematics to be considered as distinct subjects, as they are by some bodies now ? And does the reference to "two foreign languages" lower down on the same page necessarily mean two modern languages, or can Latin and Greek be included ? And finally, what are initial degrees ?

Mr. Combridge : An initial degree is the first degree a student takes, such as B.A., B.Sc. The foreign languages are distinctly meant to be modern languages, and certainly Pure and Applied Mathematics are not in this recommendation considered as two subjects.

Mr. S. Inman (County School, Isleworth) : On reading the *Report* I have two different aspects in mind. One concerns the school and the other refers to the entrance to the universities. I am in thorough agreement with the university having less control over the curriculum of the school, but with regard to the other aspect it seems there is a tendency to stiffen up the entrance examination to the university. The tendency has been to restrict the number who

want to go to universities. Is that a desirable object ? Is it desirable to have fewer entering the universities, or do we want more people with degrees ?

I also have in mind an interesting experiment carried out by one of the northern universities, in which it was shown that marks given by certain examiners were very misleading indeed. I read recently an extract from an old *Times Educational Supplement*, which dealt with an examination held in America, in Columbia University, in history, and several examiners agreed on a scheme of marking. I think 60 per cent. was a borderline pass, and any candidate near the borderline had his paper examined by the other examiners. One conscientious examiner wrote out a model set of answers in order to assist him, and, by accident, those were left with the other papers which were examined by the remaining examiners, who gave marks to this particular paper varying from 40 to 80 per cent. ! It seems to me that, as examinations are so misleading, if we defer the entrance examination until eighteen we can very easily exclude some very brilliant people from entering, especially as the age is rather late and it would probably mean the only chance for many candidates. Therefore I am against stiffening up the entrance examination too severely.

Mr. D. H. Oldham (Midsomer Norton) : There are several difficulties, one of the chief being that the discussion is reported. Another difficulty is that this is a matter which does not specially concern mathematical associations. The difficulty is an economic one. Teachers, governors, parents and children feel they have to get the School Certificate in four years, and then to get through the Higher in two years. They have to get to the university somehow, because they think it may be a means of getting an assured livelihood. Then, too, the difficulty in regard to Matric. I am getting a little tired of hearing head teachers complain about the importance that business men attach to Matric. and, at the same time, hearing or reading year after year of head masters and head mistresses, when presenting their reports, gloating over the fact that so large a proportion of their people who have passed School Certificate have passed it at the Matric. stage. It is because of things like that, that I feel it is perfectly hopeless for a body of teachers to come together and discuss this question of Matric. influence on the School Certificate. The question of the School Certificate really cannot be discussed, and reported.

Miss Stimson spoke excellently. I was entirely in agreement with everything she said, but I have a most hopeless feeling. What can we do in the matter ? Take the difficulties in a small school. I have known unfortunate people who have had to put pupils through School Certificate in history, geography, English, mathematics, and Higher School in mathematics and geography. You get this sort of thing in a small school : Two pupils, successful in getting mathematics credit in School Certificate, so that they matriculate, decide to take Higher on the Science side. Their mathematics are perfectly hopeless. When they take the Higher they fail in mathematics.

You all hear of similar cases. When these people fail in mathematics, the head-master complains to the university that the mathematics is too difficult. Nothing of the sort. The candidates are not suited. There is the glamour of the university, the glamour of degrees, and the competition between schools. Then those ghastly speech days ! Those are things that have to be deplored. That is the difficulty. It is not a difficulty that we can really discuss at all. It is a social difficulty. It is an economic difficulty. It is a difficulty of snobbery. It is not a difficulty of degrees ; practically it is a difficulty of competition for a livelihood.

The Rev. D. B. Eperson (Sherborne School) spoke of the unfortunate effects of compelling young boys, with a good number of credits in the School Certificate, to specialise in one subject, when they had not yet discovered the one branch of study most suited to their abilities ; a wider range of subjects would therefore be more valuable to them, whatever their future career might be.

Mr. M. P. Meshenberg (Tiffin's School) : I am rather interested to see that very few amongst us are prepared to rebel against the demands made upon the schoolmaster to do everything else but teach. We have business men demanding that we should prepare boys or girls for their work in life. We should teach them to write properly ; we should teach them to reckon properly is a demand of many business houses. Others insist that we should teach them to speak properly, not because it is desirable but because "it helps selling". Now we have the universities demanding that we should prepare boys and girls to take their parts properly in the university course. I for one fail to see much distinction between these various demands. It seems criminal that we should be asked to subordinate the education of the whole school to the needs of the few who will proceed to a university education. I am teaching in a school of about 550. During the year just ended we sent two boys to the university, one to Cambridge and one to London. Two out of 550 ! Two boys out of various advanced courses numbering sixty, and I venture to say that the teaching in every subject in which those two boys took part was primarily directed to obtaining entrance scholarships or admission of some sort or another to a university. The majority of the others taking the courses went to one or other of the various departments of business and industry. I contend that it is a pity that the Mathematical Association should allow itself to be misled, shall I say, into becoming the cat's-paw of this sort of agitation, and into permitting itself to advise the universities on how much more effectively they shall dragoon us into doing their job. I see no difference between asking an employer to impose his own test upon prospective employees and asking the university to impose its own test upon prospective entrants. I see no reason why the universities should not be told that all future connection between Matriculation and the School Certificate examination should be severed. Let those people who especially desire to enter on university courses take special steps to prepare themselves for those courses, just as pupils in the ordinary secondary school who

especially desire to master shorthand and typewriting take special steps to do so, and I hope that there are very few here who encourage them to make use of school facilities for that purpose. I do hope, therefore, that we shall see in the future the development of a demand on the part of secondary schools that they shall be allowed to *educate* those committed to their care; and let us leave either to themselves or to the university authorities the elaboration of schemes for selecting proper entrants to their courses.

Mr. N. F. Sheppard (Lord Wandsworth Agricultural College): That last comment leads me to suggest a new line of thought. In the past the universities have demanded of the schools certain work; perhaps in twenty years' time we shall at last be recognised at our true value and the schools will demand from the universities certain work. In the future the schools may be able to say, "We are sending out fifty boys in three years' time and we want them to study and take a degree in theatrical production" and have some power over the universities to make them provide what is wanted.

Mr. P. C. Unwin (Clifton): I recently had a conversation with an examiner who put his finger on the whole trouble with which, it seems to me, the *Report* deals, namely, that we are trying to do two things: to qualify pupils for School Certificate and to give "Distinction" in various subjects. So long as we try to do those two things we shall not produce satisfactory papers. The point seems to me to be relevant to the *Report* under discussion, and to indicate that examiners and dons at universities would probably be favourable to the proposed change.

There is one other small point. We have heard much about the examination from the point of view of the universities and from the point of view of Matriculation. I should like to put one or two points from the School Certificate angle. One of the best remarks I have seen for some time with regard to this question of a qualifying examination was that by Dr. Cyril Norwood, who, when speaking at a recent conference, said that those qualifying papers should be such that the average intelligent person could secure 100 per cent. on them. That is, to my mind, a little too drastic, but it is, nevertheless, a definite move in the right direction. It seems to me wrong to give candidates a paper of questions, some of which can be done quickly by the mathematician's low cunning, others of which have a certain amount of mathematical principle which is utterly smothered in heavy calculations which tell us nothing fresh about the boy, or nothing we could not discover by setting one or two easy straightforward calculations. I would have more and lighter questions. That reduces to a minimum the risk of the nervous or stupid boy, who does not know when to stop, having his chances spoilt by getting "stuck" in heavy calculations. Furthermore, owing to the wider field which can be covered, this does enable you to get some idea as to whether a boy has or has not grasped mathematical principles, which is really what we want to do. Making the questions lighter, more numerous, more varied, giving more intelligence tests of the kind the average boy or girl can do, is something

to be encouraged. One speaker cited the case of the intelligent boy who gets five or six credits and cannot get School Certificate because he fails in mathematics, or cannot get the right number of credits for Matriculation for the same reason. It is perfectly true that an ordinary bright, intelligent boy, who can do almost any other subject, may go utterly to pieces when it comes to anything to do with x 's and y 's. The endeavour to cram people of that type meets with failure in half the cases.

Dr. D. G. Taylor (University College, Cardiff): Perhaps I ought to apologise for speaking from the university point of view, but it seems to be in danger of going by default in the discussion. The remarks of Mr. Meshenberg are not quite fair to the general purpose of the *Report*. Though I have no official connection with it, at least in its final form, it is intended to be, and is quite obviously, I think, an attempt on the part of university teachers to meet the objections and the needs of the schools in the matter of the university and its requirements. The statement has been repeated about the universities imposing tests and requirements upon the schools. The *Report* represents a definite attempt on the part of university teachers to remove that stigma for ever. A university must have some sort of test for its own purposes. If the persons in whose hands the destinies of school pupils are for a time make a wrong use of the university test, surely the university is not to blame. The distinct point which the *Report* makes is the desire on the part of university teachers that there shall be a clean break between the School Certificate standard and the requirements for Matriculation, and I think that is the very essence of the *Report*. The standard which the Association of University Teachers desires for university entrants is not high; as a matter of fact, I do not think it has been put into a sufficiently plain statement yet, and it will require a good deal of time before it becomes sufficiently definite for practical purposes. As I take it, the situation at present is that the *Report* constitutes a gesture from the side of the university teachers towards the teachers in schools, inviting co-operation in a matter which both sides equally regard as important and indeed vital.

Mr. A. G. Carpenter (City Secondary School, Leicester): I wonder if Mr. Combridge has heard of the attempt made by one of the Examining Boards to go into this question. It produced a confidential *Report* as the result of setting up a Committee to attempt to divide the School Certificate from Matriculation altogether. The Committee recommended that Matriculation should be divided from School Certificate, that no mention of Matriculation should occur at all. But the provincial universities have for several years past been trying hard to get London University to accept their Matriculation as exemption from London Matriculation, and have succeeded in doing so. It now appears that the London University is the stumbling-block. They are so afraid that if there are other means of matriculating in provincial universities, candidates will not be able to gain exemption from the London Matriculation

examination, about which employers are so keen although they know so little about it. So for the present, although this particular Board is very keen on this separation, nothing is going to be done. I wonder if it is possible for those in control of the London University to make a similar gesture towards the separation of School Certificate from Matriculation.

Various suggestions were made in the *Report*. Summarised they are, roughly, as follows : That the present Matriculation examination be abolished, and that the granting of exemption from Matriculation on the result of the School Certificate alone should cease. In order to matriculate a person must (a) have passed a School Certificate examination under conditions obtaining at present ; (b) must have passed in English and in four subjects at subsidiary stage in Higher School Certificate, at least one subject being taken from the two lists of subjects : Arts and Science ; or (for the present) have passed full Higher Certificate examination. A candidate who passes in three out of the four subjects at subsidiary stage may be referred in that subject provided that the marks in that subject and the aggregate in the other three subjects do not fall below prescribed minima. No list of Matriculation results to be published. The lists of candidates who have qualified to enter a university to be circulated privately to the universities. Those are the suggestions in the *Report* of the Committee set up by the Board, and I think they have the support of the majority of those teaching in secondary schools.

Mr. J. T. Combridge, replying to points raised in the discussion, said : The position with regard to the Board referred to by the last speaker is, I believe, that they are still consulting their constituent universities on the subject of this *Report*. Matriculation in London is dealt with by the Matriculation Examination Council, and even university teachers who might like to see things altered are not able to effect that alteration.

I should like to underline what Dr. Taylor, of University College, Cardiff, said with regard to the remarks of Mr. Meshenberg. I accept the latter's explanation that he was not here for the opening speeches, but I also infer that he has not read the *Report*. I certainly made it clear that not only did we want to cease imposing restrictive qualifications on the schools, but what I particularly, and many like myself, would like to see is an opportunity for providing for first-year university work to fit students who come from the schools to employ their time in a university properly. We do not, and cannot, expect schools to undertake that work. I hope no one will leave this morning thinking that I or any other university teacher wants the schools to do this preparation. What I for one have been trying to do is to introduce into university work some methods such as those expounded before the Association by a speaker some two years ago. We want to move in that direction rather than in the reverse.

With regard to the other speakers in the discussion I need say nothing except that I hope we shall not allow ourselves to be

unduly influenced by any admiration of American methods in connection with degrees in salesmanship or theatrical production or other such items, or by hoary stories as to how American professors mark history papers. Someone else mentioned history, and I am reminded that I once discussed with my college tutor what subject I might take for a year after having obtained a degree in mathematics. He was inclined to turn down English because it required a particular type of mind, and then he approached the subject of history. I said, "I suppose really history requires a certain type of mind, too?" "Well, yes", he replied, "I suppose it does; although a great many people take history of whom I might almost say they have no type of mind at all."

848. " . . . and Three . "

We have always suspected statisticians to be a mean-souled, sublunary race of men, incapable of the higher things. Never has our suspicion received such striking confirmation as when we read, the other day, that: "Exactly 1,335,617,903 lbs. of chocolates and candies were consumed in the U.S.A. in 1930". Consider the implications of those figures. The last three, more especially, are in themselves an indictment of the compilers. One would have thought that the body of men—numerically, without doubt a considerable one—responsible for calculating this total would at least have shown, in the execution of their irrelevant and ridiculous task, some measure of the craftsman's pride. They would have gone all out for a round number and got one. Rather than publish a result which fell short of symmetry by so narrow a margin, and thus expose their attitude towards their life-work as a revolting compound of the slovenly and the meticulous, they should have abstained, during their calculations, from the consumption of chocolate and/or candy; then, with the total in sight, they should have made a carefully adjusted attack on a quantity of these delicacies of the 1930 vintage, (we do not doubt that the Society for Tidying up American Thought would have supplied them gratis). Such an orgy, undertaken from the most abstract and disinterested of motives, would have compelled respect. The spectacle of a roomful of statisticians eating 87 lbs. of stale sweetmeats in order to round off the national consumption figures would have been an ennobling one. But the attempt was not made. The adders remained deaf to the challenge of their art. We think worse than ever of statisticians.—*The Spectator*, June 27, 1931. [Per Mr. V. Naylor.]

849. The world to the circumference of Heaven is as a small point in geometry, whose greatness is so little, that a less cannot be made: into that narrow room, your quick imaginations we must charm, to turn that world.—T. Dekker, *Old Fortunatus*, 1600, Act ii.

850. Most sculpture, even, for example, ancient Egyptian sculpture, creates mass by a synthesis of two-dimensional aspects. We cannot see all round a cubic mass; the sculptor therefore tends to walk round his mass of stone and endeavours to make it satisfactory from every point of view. He can thus go a long way towards success, but he cannot be so successful as the sculptor whose act of creation is, as it were, a four-dimensional process growing out of a conception which inheres in the mass itself. Form is then an intuition of surface made by the sculptor imaginatively situated at the centre of gravity of the block before him. Under the guidance of this intuition, the stone is slowly educated from an arbitrary into an ideal state of existence.—Herbert Read in "*The Listener*", *Weekly Notes on Art*; *Henry Moore*. [Per Mr. F. C. Boon.]

CALCULUS AND CO-ORDINATE GEOMETRY AT THE SCHOOL CERTIFICATE STAGE.*

MR. TREVOR DENNIS, in opening the discussion, called attention to the proposed *Syllabus for Additional Mathematics for School Certificate* as circulated amongst members, and said : This syllabus has been drawn up by the Manchester Branch after consultation with other Branches, and we ask your help, your blessing and your advice as to amending it, so that we may put it before the Joint Matriculation Board of the Northern Universities as a syllabus which should stand as a separate subject. The Northern Universities School Certificate and Matriculation is a tremendous examination. Last July no less than 460 schools took the examination and 18,720 candidates were presented. It is an extraordinarily well-conducted examination in many respects, but during the last thirty years, shall we say, during which an absolute change has come over the face of mathematical teaching and the syllabuses which we teach, practically no advance has been made in the syllabus for this examination. The only appreciable advance is that we have four trigonometry questions put into the arithmetic paper as alternatives to the last four problematical arithmetic questions. The last item shown on the syllabus in your hands is the present algebra syllabus, which seems to me most deplorably restricted in amount. There is an additional mathematics paper which boys and girls may take in the School Certificate examination. The present syllabus for this paper is actually one which was suggested a few years ago by the Manchester Branch, and which is now adopted for all schools, but you will be astonished to hear that you may take this additional mathematics paper but get absolutely no credit for doing so. The Board will examine the boys or girls, send you in confidence the marks, and send you for publication the standard that the boys or girls have reached, but it affects the results of the individual examination of the candidates not a whit. Therefore boys and girls of the first school examination age, which we are told is 16 plus, are restricted to practically the same syllabus as those, who are as old as I am and went to school thirty or more years ago, took in the Senior Cambridge. The geometry is practically the ordinary modern treatment of Euclid, I-IV and VI; arithmetic goes "up to", whatever that may mean, stocks and shares, as it always did; in trigonometry four questions have been put in instead of the problematical arithmetic questions, and the algebra is the same as I had thirty or thirty-five years ago, as far as the syllabus is concerned.

We want to extend the syllabus; we want to take boys and girls further than that very small syllabus, and we are not allowed to do so. At least if we do so it is at the expense possibly of their doing well in the algebra, arithmetic and geometry. At any rate, they get no credit for it. We are thinking not only of mathematical specialists who will do mathematics in a sixth form, though person-

* A discussion at the Annual Meeting, 4th January, 1932.

ally I think it is a most excellent thing that these mathematicians should go over more advanced work the first time with extremely easy examples, so that they may understand principles and may not be fogged by difficulties of manipulation ; and then when they start in the sixth form it seems to me reasonable that they should cover the same ground again with harder mathematical questions on the same sort of syllabus. Nor are we thinking only of the scientists, though calculus may help them in mechanics, for instance, in the School Certificate. We are thinking chiefly of the regular curriculum of the ordinary boy and girl who may perhaps leave after the School Certificate, or may perhaps drop mathematics after that stage. What we want to do is to carry them further than the present, to my mind, dreadfully restricted syllabus. We want to do that by going on to this further mathematics with very easy examples ; to give them a chance to use the methods and the knowledge which they are now taught, by giving them examples which are intelligible to them, and a great deal of the algebra examples are not intelligible to anybody. They are a manipulation of symbols—symbols which mean nothing at all. It seems to me psychologically wrong that we should make beginnings in a first examination and leave loose ends and threads ; for instance, that we should introduce our pupils, at some cost of time and labour, to circular measure and be unable to answer an intelligent question as to what is the use of it ; with the present syllabus it is possible to answer all the questions set without knowing what circular measure is, except in the case of those definite pieces of book-work which deal with it.

In the same way it seems to me that the present additional mathematics syllabus, which contains circular measure and the limit of $\frac{\theta}{\sin \theta}$, does not deal with applications of it, so that when an intelligent pupil, if we happen to have one, asks us, " What is the good of bothering about all this ? " we are unable to point to anything in his own experience which is a reason for using circular measure. Again, the theory of quadratics is a terribly artificial subject—however delightful—unless you go on to, shall we say, easy analytical geometry, which deals with and uses the theory of quadratics in the way that boys or girls can understand and see the pictures of in front of their minds.

What I say next is more debateable, but I feel it very strongly. I hate teaching progressions which are in the present additional mathematical syllabus without going on to some other series. I think it more difficult to teach the beginnings of a subject and not give a boy or girl some chance of understanding it by going on to other series. I am sure we shall agree that what is psychologically wrong is difficult, if not impossible, to teach properly. We are not suggesting anything more difficult in the main. The Examination Board have been ready to meet us in the way of giving easy, straightforward questions in the first half of the arithmetic and algebra papers ; if we can restrict them to the simplest examples, in the new

paper there is no difficulty ; in fact, we shall find it easier. In their own report on the arithmetic paper they seem to be astonished that most of the candidates do not take the last four arithmetic questions, but practically *en bloc* take the trigonometry questions for the reason that is in my mind : that they are very, very much easier to do. I think that one should cover the ground more than once ; and I think that if boys are afterwards going to cover the ground in the sixth form, they will be very much better off by having been through it with the simplest examples first.

The lot of the poor man who has to set the algebra paper seems to me a very terrible one. He always gets to the end of his syllabus before he has set twelve questions, and then he has to repeat himself or make up questions which do not seem to be particularly exhilarating. In July 1930, having got to the end of his syllabus he repeated himself by putting in some more logarithms. There were two whole questions in the algebra paper on logarithms and one whole question in the Additional Mathematics paper. Otherwise he has to scratch his head and make up examples on formulae which he may possibly—I am not sure—have taken out of some engineering handbook, but I suspect were made up by himself. I would rather deal with formulae about curves which boys can study under their own eyes than deal with cumbrous expressions or complicated equations which have no meaning. And the only practical examples, if I may use the word when I mean impractical, are about *A* and *B* running races and *C* being left at the post, etc., and so many boys going on excursions : if three had not gone, etc. I would much rather take the boy on to something that is mathematics. I cannot conceive of any real difficulty in principle about the syllabus as suggested, nor do I expect that mathematicians would find one. There is one practical difficulty, and that is that there may not be in some schools any mathematicians who can teach it. I am thinking of the smaller schools, in which a man has to teach drawing, a little drill, mathematics, German and French. Surely it is our business to assist in providing the mathematicians whom, I am sure, will be forthcoming.

But now one comes to the procedure. We really want your help. I am not simply talking to instruct you. We are here to ask for your help. I understand that the procedure is for you to approve of the syllabus ; we want your advice as to any alterations in it, but, if you approve, something which I hope you will do is to recommend that it should go to the Teaching Committee of the Association.

The Chairman : I do not think a seconder is wanted. The proposal is :

“ That a copy of the syllabus as desired should be sent to the Secretary of the General Teaching Committee ”.

Mr. A. W. Siddons (Harrow) : I am very interested in this question, and as a result of conversations I recently had in Manchester, I wonder whether under “ Trigonometry ” in the suggested syllabus “ two-dimensional problems ” should not read “ three-dimensional problems ” ?

Mr. Trevor Dennis : When I copied out the syllabus which was sent me by the secretary of our Branch I wondered the same.

Mr. Siddons : My recollection in talking it over was that "three-dimensional problems" were intended.

Mr. A. Dakin : I think it should read "simple problems".

Mr. Siddons : When speaking to your Branch in Manchester, I talked the matter over with Miss Garner. I think it would be a great pity if you did not include "three-dimensional problems". You are probably already familiar with the papers set in the Oxford and Cambridge Joint Board School Certificate. They have been setting good papers lately. One paper on algebra, geometry and trigonometry (the geometry has been usually on straightforward work in three dimensions), and another paper on calculus and co-ordinate geometry. During a fair number of years I have been concerned with boys doing that work very much on the lines of the proposed syllabus, except that we do not go so far as differentiation of trigonometrical functions. It has been most illuminating to boys, some of whom have become mathematicians, some classics and some historians. I think that the syllabus will be most valuable, and I heartily wish you success in getting it carried into effect. It seems desirable that something of the sort should be achieved. The introduction of a little three-dimensional work would be an improvement. I would also be inclined to suggest under Cartesian co-ordinates having a little about the centre of the circle. It is dangerous, perhaps, but the work I should like to see does not involve any formulae, but merely common sense. As long as you keep off the inclusion of technical things, that work is quite nice. On the whole, the syllabus is just about the standard of the Oxford and Cambridge School Certificate, except that that does not include differentiation of trigonometrical functions. I think possibly, when it is being put before your Board, some good papers that have been set by Oxford and Cambridge could be sent in with it.

I strongly object to the introduction of the integral notation until a boy is introduced to the idea of an integral as the limit of a sum. This syllabus is indefinite on the point. Most are. On two occasions, the Oxford and Cambridge Joint Board has set papers involving integral notations ; with those two exceptions, they have avoided the integral notation. I feel strongly that when you have introduced the boy to the idea of a sum he may use integral notation ; but before that he can get on quite well without it. I think that the proposed syllabus implies integration simply as anti-differentiation, which is quite good enough for the stage which you want : areas under curves and volumes of solids of revolution.

Mr. Hope-Jones (Eton) : I should like to support as strongly as I can Mr. Siddons' plea for the early introduction of solid geometry, including trigonometry in three dimensions, to everybody who learns any geometry or trigonometry at all. It seems to me that one of the most disgraceful pieces of inertia on the part of the human mind, grown-up and youthful, is its limitation to two dimensions. One of the things which we most need to force ourselves to do, and

to teach children to do, is to pull their minds out of two dimensions into three. I would certainly recommend that any course on trigonometry should include three-dimensional problems before coming to the addition formula.

Mr. A. H. G. Palmer (Whitgift Grammar School) : I should like to ask what relation this has to the mechanics syllabus. Personally I think it is a pity to consider subjects like trigonometry and calculus for the Additional Mathematics apart from their relation to mechanics.

Mr. Trevor Dennis : There is a mechanics paper in the Science Group, and candidates can get credit for their knowledge of mechanics. You would suggest mechanics in this?

Mr. Palmer : Yes, I think they should be side by side.

Mr. Trevor Dennis : I think the reason why it is not included is that at present a candidate can get credit for his knowledge of mechanics, but not credit for his knowledge of anything in the proposed syllabus.

Mr. Palmer : What about extending this scheme to cover the mechanics syllabus?

Mr. Dennis : We have not attacked it yet.

Mr. Palmer : Can you tell us a little about it. Does it go into two dimensions, triangle of forces, and relative velocity, or is it confined to kinematics and dynamics of motion in a straight line?

Mr. Dennis read the items enumerated in the three-hours' mechanics paper.

Mr. A. Dakin (Stretford) : May I say a word or two about the suggested additions to the syllabus? First of all, we do desire to introduce new ideas into the mathematics syllabus of the School Certificate stage, but we are not forgetting the fact that if we overload this syllabus it has a very good chance of being turned down not only by the Board but also by the schools who take the Joint Matriculation Board's Examination. We had to bear that in mind when drawing up this syllabus. **Mr. Dennis** will perhaps allow me to make a slight adjustment of his statement about the present syllabus. Some years ago the Additional Mathematics Syllabus of the Joint Board consisted only of algebra and trigonometry. Nine schools around Manchester got together and drew up a special syllabus, including the calculus, and submitted this to the Board as a special syllabus under their regulations, and it came to be accepted for those nine schools. Two years later, in 1928, that syllabus was the official syllabus of the Joint Board. That is the way in which we introduced calculus into the Additional Mathematics syllabus of the Joint Board. But the present syllabus is not the one which was drawn up by the Manchester Branch. Two years ago, to our very great surprise, not only the syllabus but the nature of the papers were changed by the Joint Board without warning to the teachers in the neighbourhood, and we regard these changes, particularly the change in the type of paper, as a retrograde step which is having a very bad effect upon the teaching of mathematics in the schools of the North. If you consider the paper now you will

find that a boy may only do two questions in the calculus. The paper is so divided that he is bound to take five questions in algebra and trigonometry, and can only take two in the calculus. That is contrary to all we desire. The questions in the algebra and trigonometry are very much of the type of the ordinary School Certificate course. What we do feel is that this Additional Mathematics should widen the horizon of the boy by introducing to him new methods and by giving him a new outlook. We do not wish the Additional Mathematics syllabus to consist of harder questions in algebra and in trigonometry. What we do want is to show the boy, before he leaves school at sixteen, that mathematics has a wider outlook, that there is a broader and longer road that he can tread without going backwards and forwards over the same old little bit of land. That is our object, primarily, in introducing this syllabus. I do not think we thought of introducing solid geometry, but we welcome the suggestion. The question of introducing pure geometry was brought forward and discussed. It was turned down because we felt that it did not lead to any of the fields that we wished the pupil to be given an opportunity of exploring after he left school, or after he had passed from the School Certificate stage to the sixth form. I do not think we should turn down solid geometry on that ground. I think we should be willing to accept a certain amount of solid geometry in this syllabus if the Teaching Committee thought it desirable. I am perfectly certain that when the syllabus was drawn up, none of us intended that three-dimensional problems in trigonometry should be excluded. We all thought that simple problems in trigonometry included three-dimensional as well as two-dimensional problems.

As regards the question of mechanics, it must be remembered that if we overload this syllabus it has a very good chance of being turned down not merely by the Board but by the schools of our own district. At the same time, the syllabus is left, I think, sufficiently vague to enable a good teacher to give examples from mechanics in teaching these fundamental ideas of the calculus. In the same way, with reference to Mr. Siddons' remark about an integration notation, it is not suggested that the integration notation shall or shall not be used. It is left open for the teacher to select his own method.

And finally, I want to say that those on the Committee attach considerable importance to the type of question that will be set on this paper. We do not want the old type that calls for a great deal of manipulative skill. We want the type of question which tests whether the boy has or has not grasped the new ideas of elementary analysis that we are trying to introduce to him in this syllabus, and we want in putting it to the Board the support of the whole Association. Yesterday we were told that the machine is ample for bringing the barrage of the whole Association to bear upon the Joint Board. I hope the Teaching Committee will accept this syllabus, and will present a real and effective barrage or attack upon the Board and make them drop the present type of paper which, to my mind, is

entirely retrograde and doing considerable harm to the teaching of mathematics in the North.

Mr. A. Montagnon (The Grammar School, Leeds) : It seems to me that there are some difficulties that will have to be overcome. I understood Mr. Trevor Dennis to suggest that this syllabus should become a full subject for the School Certificate Examination. Was I right ?

Mr. Trevor Dennis : Yes, that is most important.

Mr. Montagnon : It seems to me that if that suggestion is carried out it will pave the way for very great opposition from very many schools, because of the fact that it will be more or less granting to the subject of mathematics a pre-eminent position in the School Certificate, in so far as a boy will be able to present two complete subjects on mathematics in the group of Science. It seems to me possible under the Northern regulations for a boy then to obtain a School Certificate on a pass in English and in one language and in three subjects of a mathematical type—ordinary mathematics, mechanics and this additional mathematics—and I think there will be teachers of other subjects who would strenuously oppose such a suggestion.

Furthermore, there seem to me to be difficulties in the ordinary way in connexion with this being built up on the school and its curriculum. If you are going to present boys for this Additional Mathematics you would normally have to have a school where your boys are very well sorted up in the fifth forms. If your school is not so adjusted, you will have, when they get into a sixth form, the difficulty of overcoming the one type of student who has had no further mathematics beyond the algebra and trigonometry now normally presented and the other type which has already gone through this course.

Again, what real advantage is this serving ? It seems to me at present, without very great study of it, that if a student is very anxious to present a qualification of attainment of this type, he can quite easily do it in his first year in the sixth form by taking the subsidiary mathematics of the Higher School Certificate, and that seems to me not to upset the existing conditions in schools and will be a factor which will carry very great weight with very many of the schools.

Reference has been made to trigonometry, as it plays an important part in this scheme, and we have been told of the great fact that the Northern Board give, say, four questions in the arithmetic paper. I do not know whether it is common knowledge, but those four questions are going to be reduced to two. And so it seems to me that if you are to carry this through you will have a bigger fight than has, perhaps, been at first sight estimated.

Mr. Tuckey (Charterhouse) : On behalf of the Teaching Committee I welcome the suggestion that we should have this piece of work to discuss. I think that a good deal of discussion will be required, and I would suggest that it would be a good plan, which I think we can carry out under our Constitution, if we co-opted as

members of the Teaching Committee, for the occasion at least, one or two representatives who drew up this syllabus. I suppose that would be possible ?

The Chairman : Oh, yes.

Mr. Tuckey : We shall, no doubt, therefore have plenty of opportunity of discussing it. I would suggest that it would be better if we were allowed not merely to take or leave it, but to discuss details a little.

Mr. Trevor Dennis : I asked for your advice.

Mr. Tuckey : Speaking on behalf of the Teaching Committee, we welcome the opportunity and will be glad to do our best in the matter. Speaking as a teacher at Charterhouse, the whole thing seems complicated to me, because the additional mathematics which we teach for School Certificate is so entirely different from what we are discussing. We take the Oxford and Cambridge Joint Board papers, and we have two papers, the geometry, algebra and trigonometry and the mechanics. There is also an optional paper on analytical geometry and calculus, which we find we have not time to take, apart from an occasional boy. The result is that our teaching of additional mathematics at the School Certificate stage is almost entirely the teaching of mechanics, or very largely that. We do a little teaching on the geometry, algebra and trigonometry, but when we think of Additional Mathematics, we think of it as mechanics. I am not at all sure that mechanics does not imply a broadening of the mathematical outlook and a bringing of it into touch with life more than this extension to analysis and calculus. I do not know whether there is any suggestion that mechanics should be introduced as part of the Additional Mathematics for the Northern Examination. Also, it seems to me a little surprising that there should be the one paper carrying the same credit as a larger number of papers in other branches. It seems to me, on the whole, the more natural arrangement is that there should be at least two papers to a subject in which it is possible to get a credit.

Mr. Trevor Dennis : We put down one paper of three hours, three hours being the usual time for credits. Personally, I would welcome Mr. Tuckey's suggestion that there should be one on these lines, perhaps watered down, for one and a half hours, and one on mechanics for one and a half hours.

Mr. Tuckey : That is a point we might discuss in the Teaching Committee. Perhaps you know which recommendation will be likely to be accepted by the Board who settle the matter.

Mr. F. Sandon (Plymouth) : May I ask if it is proposed to alter the present syllabus in any way, either in connection with trigonometry or algebra ?

The Chairman : No, not at all.

Mr. Sandon : In that case I notice that you have "Maxima and minima" in Elementary Analysis, and in Algebra "Algebra to quadratic equations including graphs of linear and quadratic functions", and yet nowhere can I see that you even graph a cubic.

Mr. Trevor Dennis : I should not be surprised if cubic graphs were set on the present syllabus. We propose cubics, and in fact any curve of the form $y=f(x)$, where $f(x)$ can be differentiated by the candidate.

Mr. A. Montagnon : I think there is a little misunderstanding there.

Mr. Dennis : Down to the line across the page on the paper distributed to members is the proposed syllabus for Additional Mathematics. Below that is the trigonometry and algebra of the present syllabus—not Additional Mathematics at all. There is not a word here about the present Additional Mathematics paper—it is too horrible to print!

Mr. F. Sandon : That is what I understand. Please tell me : is it your proposal to modify the present ordinary mathematics in any way?

Mr. Trevor Dennis : We have not attacked that in any way yet.

Mr. Sandon : In that case you are proposing to carry on with a scheme in which boys can do graphs and quadratics in the elementary paper, and saying they can attack maxima and minima in elementary analysis without even having graphed a cubic.

Mr. Trevor Dennis : We suggest, broadly, the curve $y=f(x)$, where $f(x)$ can be differentiated by the candidate.

Mr. Sandon : That is simply the tangent and the normal. My point is that simply graphing a cubic is considerably simpler than maxima and minima or "tangent and normal at a point to curve $y=f(x)$ ". Simple things like that are not provided for. Is it your opinion or your wish that questions such as graphing cubics should be in the Additional Mathematics paper or in the ordinary mathematics paper? I am wondering whether you propose to modify the ordinary mathematics paper in any way, or whether you require graphs, cubics and so on to come in as additional mathematics.

Mr. Trevor Dennis, answering questions : I am afraid we have not dealt with the ordinary mathematics paper yet. Personally, I should not feel aggrieved if in the present paper they set a cubic graph, though it seems they are not entitled to do so. We certainly hope that in our proposed syllabus for the Additional Mathematics such curves would be set and dealt with ; in fact, curves of any function that candidates can differentiate.

Mr. Meshenberg (Tiffin's School) : I should like to bring to your notice points put to me in a discussion I had on calculus with some of the more able members of the sixth form. They had had one year's calculus only, and had covered probably a little more than the ground outlined here, and, in all probability, had done a little more within that ground also. Their question to me at that stage was what was the object of the calculus. Now, as it has been put to us, the calculus is suggested as an additional subject for part of the additional paper in order to broaden the outlook of a student of mathematics. It seems to me an even more sharply pertinent question when I am asked : "What is the use of the calculus, which is supposed to show me more about the uses of mathematics?"

When I look at the integral I find it is given "as the inverse of differentiation", and we are to find "areas under curves and volumes of solids of revolution". Now I ask you whether that is not, after all, asking the pupils to master a little trick whereby they are shown that owing to the fact that by a certain special process you can show that the function, whose differential coefficient is a certain thing, gives you your area, therefore you have solved the problem when you have inverted your differentiation. It is a trick, and those boys were clever enough to ask me, "Is not it more practical to find your area of the curve, if that is your object, either by graphing or approximation, than to find your area of the curve by some sort of trick?" The worst of it is that boys intending to become engineers are expert in knowing how to find areas, and they challenge me to find them as easily by the calculus as by the method they use. I do not accept the challenge.

What I am getting at is really the point made by one or two speakers. You want to broaden the outlook of these boys at a very difficult stage, the School Certificate stage, when they are already finding it pretty hard to do what they are asked to do. We are constantly complaining of the difficulty of getting these boys to satisfy present requirements. In fact, we have already had discussions which aim at simplifying the present requirements, and we are already finding that the university requirements in the way of credits on present papers are too much for us. And yet we want to offer them calculus, and even have a suggestion, in which I am in great sympathy with Mr. Siddons, and which gives the game away, that a boy should not be allowed to use the sign of integration until he has got to the limit of a sum. I heartily agree, but I think he implied that the boys at School Certificate stage could not be expected to look at it from that point of view, and I would urge that they cannot look at the differential coefficient from the point of view of a limit. I am so strong in my feeling on that point that I have long ago come to the conclusion that I should not like to teach calculus to School Certificate stage. I used to be very boastful about the fact that I could make $\frac{dy}{dx}$ perfectly clear, but I have since questioned men, who have London Degrees, on the subject, and they have admitted that they were not perfectly sure whether the $\frac{dy}{dx}$ was the slope of the tangent or whether it differed by something that did not much matter. I put it to you that if such a state of affairs is in existence—and I give it to you as a fact that it does exist—with respect to $\frac{dy}{dx}$, we ought to avoid the calculus, teaching them gradients, if you like, and be candid and say: "There is a dodge which if you follow along these lines gives the right answer—I am not here to justify it at this stage, but if you are interested, follow on". I have done that frequently and quite shamelessly. I have often given them dodges that work, and told them that the

justification of the dodge is something too difficult at that stage. I regard that as honest, although from some points of view immoral!

I hope my remarks will have made clear my attitude towards the calculus. As for the rest of the syllabus, progressions are in some cases all right, but I do not see the object of "Arithmetico-geometric series". If the proposers of the syllabus are anxious to add to the boys' box of tricks, this is rather a pretty dodge and the boys enjoy it, but if you told me it broadened their outlook, I should want more information upon how it does that.

The only part of the syllabus with which I am in hearty agreement is the trigonometry. There I am absolutely in agreement. I loathe the wording of the present syllabus. Whenever I see "trigonometrical functions of one angle", I always have a horrid vision of the various tricks sprung upon candidates in examination papers under that head.

I conclude by urging that the Mathematical Association does not identify itself with the scheme for urging the calculus for the School Certificate stage, although they may recommend it for the abler boys who intend to follow mathematics in the sixth form.

Mr. Katz (Croydon): Having already taken part in acute controversy, I should not have spoken, but I am encouraged by the speaker from Leeds, who put what is my own position with regard to this matter. I take it that he has no objection to the syllabus as such. I suppose he would regard it with me and with Mr. Siddons and others as a very considerable improvement on the earlier syllabus; that is to say, as a syllabus for additional mathematics it is excellent. But the point is that once you have adopted the syllabus you are faced with an additional subject for examination. That is the real issue. I observe that the opener said that he had presented pupils in additional mathematics on the older syllabus, but the subject does not count for examination credit. The fact that no credits were obtainable was an excellent arrangement. There are many in this audience who (with me) do not regard that as a defect at all. For several years I sent in boys for the additional mathematics paper of the London School Leaving Certificate, but during the last few years I have dropped it. My reasons were these. In the first place, I found you could only send in boys for the additional paper if you were working with an A class; you could not do it with a B or C class. Further, if you sent in the whole class for this additional paper, you found that a good 20 or 30 per cent. were worried by the requirements of the ordinary papers. If, then, you persist in sending in the class as a whole for the additional paper, you have to settle down very seriously and work up a quantity of extra trigonometry, calculus and the rest. In fact, you are forced to cram. The calculus is a fascinating subject. Why spoil it for these boys?

This does not mean that I do no calculus in the matriculation year. Many boys leave school after taking the School Leaving Certificate, and it would be a pity if they had no introduction to the calculus. With many other teachers I introduce calculus to

boys in the matriculation year. But we do not ask these boys to take it as an examination subject ; and, seeing that examinations have been our bane, I am rather surprised that gentlemen from the North seem so eager to have additional examinations fastened on to them. By all means let us have this syllabus. Better judges than I have said that it is a very good syllabus, but do not let us ask even our A forms to be examined on it.

Mr. N. F. Sheppard (Lord Wandsworth Agricultural College) : I am looking at this matter from the point of view of a teacher who has had engineering training. I have seen boys from the ordinary public schools trying to apply to practical matters the mathematics which they have learned, and I feel strongly that the work done in the school should not be in any way remote from practical experience. I should like to see much more solid geometry done, and a widening of the whole concept of solid geometry as part of the advanced mathematics in the school. I should like differentiation to be usually with respect to time, and calculus to lead up to the differentiation, for example, of electric force with distance, rather than of abstract functions. I feel that anybody taking up more advanced work should have a very wide and firm basis in the application of mathematics to things which he can see and feel, and further mathematical developments could and should await the university stage.

Mr. A. H. G. Palmer (Whitgift) : I think the following suggestion would meet a good many of the points raised this afternoon. Cut out the distinction between this additional mathematics and mechanics, and regard the whole thing as one subject. That would reduce the number of extra subjects which a boy might take from two to one, and it has further advantages. I cannot envisage the teaching of elementary calculus without at the same time thinking of the teaching of elementary mechanics ; the two subjects help one another so much in the early stages. Mechanics is also of great assistance to a boy in his algebra. The constant acceleration formulae, to mention only one instance, afford valuable practice in algebra, and for that reason I am one of those who are addicted to their use.

It would be wise to avoid any suspicion of pressure, for I agree with Mr. Katz that we must not lend ourselves to cramming. I suggest, therefore, that there be two papers, say of two or two and a half hours each, both of them mixed, and including trigonometry, calculus, mechanics, algebra, and geometry. They should be mixed in order to leave a master free to exclude one subject if he wishes without seriously prejudicing the boy's chance of passing. At the same time the arrangement would tend to encourage the teaching of all three new subjects, a respect in which it would be better than, for example, the Oxford and Cambridge Joint Board, whose papers seem to me to call for the extensive teaching of two subjects and entire exclusion of the third.

As regards syllabus, I should like to see mechanics confined to the kinematics and dynamics of motion in one straight line. That

contains all the valuable ideas—force, energy, and momentum. With regard to trigonometry, I agree very much with what is suggested here, except that I should prefer the addition formulae to be omitted. In calculus, roughly the differentiation and integration of x^n with applications of all kinds. If progressions are not in the syllabus for elementary mathematics, by all means let them be included, together with indices and simple theory of logarithms. I think that that syllabus could easily be covered, even in schools where the sets are large and not too well graded, without causing any cramming at all.

Mr. Steven Inman (Isleworth): There are a number of misconceptions as to the aim of this suggested syllabus. Mr. Dennis appears to think that the suggested syllabus should be a compulsory one.

Mr. Dennis: Optional absolutely.

Mr. Inman: The previous speaker said that there was a possibility of some two or three subjects, but if you examine science papers you see in most universities there are papers on heat, light and sound, mechanics, chemistry, the terrestrial magnet, botany, and so on. There are at least five—probably more—subjects. Therefore I think that as advanced mathematics is an optional subject, we cannot grumble on that score. Then, again, I do not think a large number of people do take the subject. I do not think more than about 5 per cent. of candidates who offer the more elementary mathematics also offer the more advanced paper. The question is whether this syllabus is better than one which requires a lot of manipulation, whether this is wider and more important educationally, or is it one which requires what I consider to be to a large extent a lot of low cunning in order to get out a solution? I also submitted candidates for the more advanced mathematics in a number of examinations, but after a while I considered it a waste of time and gave it up, and probably there is some reason for that view.

Mr. A. Dakin (Stretford): In discussing this syllabus I hope the Teaching Committee will remember two things. The first is that in this question of mechanics, although it is taken in boys' schools, we do hope that the syllabus as it is at present will be taken by a number of girls' schools in Manchester and district. I can assure you that it is not easy to get some of the girls to take mechanics at School Certificate stage. There is, I think, adequate provision in the Joint Board syllabuses at present for boys who wish to take mechanics at the School Certificate stage. Then I hope that a distinction will be made between the two points, the adoption of the suggestion of this syllabus, and the making of this a credit subject in the matriculation examination. Personally, I do not care whether it is a matriculation subject or not—many of us are not very keen about that matter—but what I do care about most earnestly is that the old syllabus, whether it is taken by the many or the few (and the argument that only a few people take it, and therefore it does not matter what the syllabus is like, is a vicious one), and the old

type of questions should be considered by the Teaching Committee, to see if they cannot back up the Manchester Branch and bring about a very considerable improvement in the existing syllabus. The question of whether additional mathematics should be a matriculation subject or not is subsidiary.

Mr. Trevor Dennis : One or two points have been raised in this discussion which require an answer. I do think that three hours is rather long for a paper, but that is the present habit. I should welcome two papers, each of an hour and a half, and I should welcome mechanics also. Do let me emphasise that this would be an entirely optional paper. Personally, I do not think it will do a great deal of good unless it is a credit subject, like the other subjects of the examination, because if it is not, very few schools will take it. My own case is that I had to let a lot of boys off taking additional mathematics so that they could get credit by taking art. It is vital that these boys should get through their examinations. The question whether additional mathematics should or should not count as a full subject does not seem to me to be really a question for the Mathematical Association. I cannot see why this Association should thrust these rocks in front of us. There will be other people who will do that for us. But I am well aware of these troubles coming. If you refer this matter to the Teaching Committee, I ask them to consider a syllabus suitable for a full subject. The question whether it is accepted or not as a full subject must be dealt with elsewhere. If you approve of this as a full subject, then we can attack the Joint Board, and tell them the Mathematical Association approves of it as a proper syllabus for a full subject. Most of what one of the speakers said seemed to me to be dealing with rocks ahead, which I do not think we need bother about until the time comes when we have to face them. I am perfectly certain that if we are to help on mathematics in England we shall be doing a great thing by offering boys a full subject on these lines. At present schools will not take it; they will then do so. I am sorry to hear there are to be only two trigonometry questions in future. Where there are three sets, probably the A set first, and the B set later, would take this additional paper, and the sets that take it would certainly include those boys who were going on to sixth form work in mathematics. Again let me emphasise the fact that this is entirely optional. I cannot understand why it should be better that there should be no credit given for a subject, that one should do it quite apart from examination requirements. As a headmaster, it is my experience that subjects so dealt with go to the wall at once. In conclusion, may I propose that this matter be referred to the Teaching Committee?

A Member, on a point of order, asked that it might be made quite clear that this scheme was not to be placed before the Teaching Committee as a separate subject for the School Certificate.

The Chairman said that that was a matter which could be settled by the Teaching Committee itself.

Mr. Katz asked whether it was necessary to have any vote at all. There had been a very friendly discussion, various people had made

their observations, and no doubt, in the light of the discussion, the Teaching Committee would be able to deal properly with the whole matter.

The Chairman said that he thought there ought to be a vote, otherwise the thing would seem to be left unfinished. He would like to gather it up and see whether the meeting approved of the proposed way of doing it. A rider could be added on any particular point, of which it might be desired that the committee should take cognisance. One such point had been raised by Mr. Hope-Jones, and referred to by one or two other speakers, namely, the question of whether this should be amended by bringing in three-dimensional work. He did not know whether the meeting would like to vote on the question that that should be added or not, or whether the Teaching Committee should be left a free hand.

By general applause the meeting agreed to refer the whole matter to the General Teaching Committee, leaving it entirely to their discretion, without binding their hands in any way.

851. Quand les données d'un problème peuvent être placées par rapport aux axes coordonnés, de manière à simplifier les équations, sans en troubler la symétrie, il faut toujours en profiter ; mais si cette simplification de position anéantit cette même symétrie, en détruisant les quantités qui pourraient la faire apercevoir, elle n'est plus qu'apparente, et il vaut mieux placer les axes coordonnés d'une manière arbitraire. Si l'équation finale est plus longue, la similitude de ses termes, non seulement donne le moyen de la retenir plus facilement quand l'importance de son usage l'exige, mais encore procure l'immense avantage de pouvoir y lire plus facilement la réponse à la question proposée. C'est une phrase longue sans affectation, qu'on doit préférer à une plus courte, mais souvent moins intelligible.—Lamé, *Examen des différentes méthodes employées pour résoudre les problèmes de Géométrie*. § 19, p. 27. (1818 ; facsimile reprint.) [Per Prof. E. H. Neville.]

852. Toutes les fois qu'il s'agit de résoudre deux problèmes analogues, l'un dans l'espace, l'autre sur le plan, il vaut mieux commencer par résoudre celui de l'espace ; souvent on en déduit rigoureusement la solution demandée sur le plan, tandis qu'en résolvant d'abord la question la plus simple, on ne ferait que deviner l'autre par analogie. Il y a même quelquefois de l'avantage à traiter d'abord le problème de l'espace analogue à un problème proposé seulement sur le plan ; par exemple, on ignorerait beaucoup de solutions élégantes du problème du cercle tangent à trois autres, si leurs inventeurs ne les avaient été chercher dans les sphères. Il est vrai qu'en généralisant ainsi un problème, on peut le rendre plus compliqué ; mais aussi cette généralité donne-t-elle une solution plus applicable à tous les cas particuliers de l'énoncé. C'est, pour ainsi dire, une question d'Arithmétique résolue par l'Algèbre, pour obtenir une formule, où se trouve écrite la réponse à toutes les questions semblables.—Lamé, *Examen des différentes méthodes employées pour résoudre les problèmes de Géométrie*. § 23, p. 50. (1818 ; facsimile reprint.) [Per Prof. E. H. Neville.]

853. Nous nous servons, lui repartis-je, de l'astrologie comme vous vous servez de l'algèbre. Chaque nation a sa science, selon laquelle elle règle sa politique ; tous les astrologues ensemble n'ont jamais fait tant de sottises en notre Perse qu'un seul de vos algébristes en a fait ici. Croyez-vous que le concours fortuit des astres ne soit pas une règle aussi sûre que les beaux raisonnements de votre faiseur de Système ?—Montesquieu, *Lettres Persanes*, xxxv. [Per Mr. J. B. Bretherton.]

SQUARE ROOTS AND OTHERS.

BY C. V. BOYS, F.R.S.

IN Sir Thomas Heath's entrancing *Manual of Greek Mathematics* it is stated on p. 52 that the Babylonians discovered "a most perfect or musical proportion" between two numbers, which Pythagoras introduced into Greece. This is the ratio $a : \frac{1}{2}(a+b) :: 2ab/(a+b) : b$. The two middle terms are the arithmetic and harmonic means of the extreme terms. Not knowing of this when considering the fundamental principle of the logarithm, it seemed to me that the use of these two means should assist in evaluating the natural logarithm of a ratio in which the numerator and denominator are the first and last terms. In this way I found the surprisingly exact but approximate expression for the numerical value of such a logarithm which appeared in *Nature* of 14th March, 1931.

While calculating a correcting term for this expression the quantity \sqrt{ab} obtruded itself among a number of a 's and b 's and being intractable I had to resolve it somehow even if only approximately, and this led to a process which, applied to plain numbers, has great advantages over the standard method of the books.

I do not for a moment pretend that the method has not been discovered before, but it is certainly not generally known,* and it is possible that I am not the only member of the Mathematical Association who has not known all about it ever since he was at school ; and it may have some interest for members as an alternative school item. It has a further interest in that where the square root of a number is required for any purpose to a degree of accuracy far beyond seven or eight digits, it provides the quickest method I know for obtaining it. Even with Flower's method of finding logarithms to twenty places, supposing the special tables to be available, the process is too tedious to make it preferable to that which I am about to describe. This is best explained by an admittedly favourable and simple example.

* Mr. Broadbent has kindly drawn my attention to Whittaker and Robinson's *Calculus of Observations*, p. 79—The Principle of Iteration.

This is absolutely the process that I am now describing but with this important difference. The authors of the book and presumably those previous writers to whose work they refer, never appreciated the importance of discarding decimals. When more than a year ago I used the process, which I had independently hit upon for finding the approximate square root of an algebraical quantity, also upon plain numbers and naturally used decimals, I saw at once that the process was nearly as exhausting as the method universally taught in the schools and was quite unsuitable where a root was required with a very high degree of accuracy ; but that if I kept to vulgar fractions I obtained the advantages herein described. Perhaps eminent mathematicians would not naturally use vulgar fractions, and it needed someone much more prosaic to descend so low as to prefer them vulgar or even improper. However, the change over has converted a relatively useless process into by far the best that I have seen, and whereas in either, a mistake does not alter the result but only delays its attainment, the vulgar fraction method has the further advantage that the difference rule for the numerators shows if a mistake has been made and where, and Barlow's Tables provide much of the arithmetic ready done.

To find $\sqrt{10}$. Take the nearest fraction to the square root that can be found by inspection.

$$(3\frac{1}{6})^2 = 9 + 2 \cdot 3 \cdot \frac{1}{6} + \frac{1}{36} = 10\frac{1}{36}.$$

So $3\frac{1}{6}$ or $\frac{19}{6}$ is slightly in excess of $\sqrt{10}$. Divide 10 by $\frac{19}{6}$ and get $\frac{60}{19}$. This will be slightly too small. Take the arithmetic mean of these numbers.

$$\frac{1}{2}(\frac{19}{6} + \frac{60}{19}) = (19^2 + 10 \cdot 6^2)/2 \cdot 6 \cdot 19 = (361 + 360)/228 = 721/228.$$

This arithmetic mean is necessarily just in excess of the true square root, but is much nearer to it than either of the first approximations. Divide 10 by 721/228 and get 2280/721, the harmonic mean. This will be too small. Again take the arithmetic mean between these two :

$$\begin{aligned} \frac{1}{2}(721/228 + 2280/721) &= (721^2 + 10 \cdot 228^2)/2 \cdot 228 \cdot 721 \\ &= (519841 + 519840)/328776 \\ &= 1039681/328776, \end{aligned}$$

the second arithmetic mean ; now take the second harmonic mean
 $3287760/1039681$.

In the same way the third and higher means may be found if desired. But this will not often be the case, since (in this case) :

the error of the first arithmetic mean is 1 in 10^6 ;

the error of the second arithmetic mean is 1 in $2 \cdot 10^{12}$;

the error of the third arithmetic mean is 1 in 10^{25} ;

and the error of the fourth if found would be about 2 in 10^{51} .

This method has in practice great advantages over the stereotyped and dreary school-book method of finding square roots, as it doubles the number of correct figures at each operation instead of adding merely one digit. It does not so easily conduce to arithmetical error, and it has the advantage that at each stage the two parts of the numerator and the two means confirm one another and a cumulative error is impossible. In the case given, the two parts of each numerator differ by 1 in the first mean and by 1 in each succeeding mean. Had $3\frac{1}{7}$ been taken to start with, the difference in the first numerator would have been found to be -6 , in the second $+36$, in the third 36^2 , and so on, squaring the last difference for each new mean.

Where a vulgar fraction is being operated upon for its root by this method the sequence of differences in the numerators is modified. If d is the first difference found in the numerator and p is the denominator of the fraction being operated upon, then the second difference will be d^2p , the third d^4p^3 , and so on. Thus for $\sqrt{\frac{3}{7}}$ with $\frac{2}{3}$ as approximate value, the differences are 1 ; $1^2 \cdot 7 = 7$; $1^4 \cdot 7^3 = 343$, and so on. For $\sqrt{\frac{5}{13}}$ with $\frac{11}{13}$ as approximate value, the first difference is 2 ; the second $2^2 \cdot 7 = 28$; the third $2^4 \cdot 7^3 = 5488$, and so on.

The truth of this general rule may be shown algebraically. If $a/b = \sqrt{m/p}$ approximately, the first convergent will be

$$\frac{1}{2}(a/b + mb/pa) = (pa^2 + mb^2)/2pab.$$

If d is the first numerical difference, then

$$pa^2 = mb^2 + d,$$

so the second convergent is

$$\frac{1}{2} \left(\frac{2mb^2 + d}{2pab} + \frac{m}{p} \cdot \frac{2pab}{2mb^2 + d} \right).$$

If this is simplified, remembering *not* to divide numerator and denominator by p , it will be found that the numerator becomes

$$p[(4m^2b^4 + 4mb^2d + d^2) + (4m^2b^4 + 4mb^2d)],$$

and the difference between the two elements is pd^2 .

In a third convergent this becomes the d of the previous one, so the new difference is $p \times p^2d^4 = p^3d^4$. Similarly p^7d^8 is the next, and so on. Or, if both numerator and denominator be divided by p or its numerical equivalent then p will disappear altogether from the differences.

If the two elements of the numerator had been equal the square root would have been perfect. The quotient d/pa^2 or d/mb^2 in the first convergent is a measure of the approximation : $d^2/4m^2b^4$ is a corresponding measure in the second, and this is the square of half the previous one. Hence the rapid convergence and the value of a good start.

The arithmetic is shorter too than the preliminary statement might seem to suggest. No arithmetic mean is actually obtained by effecting the long division indicated. The fraction representing any arithmetic mean is used only to find the corresponding harmonic mean, and the harmonic mean only is found by division directly. It may be worth while to give the actual figures for $\sqrt{10}$.

First mean is $\frac{1}{2} \left(\frac{1}{6} + \frac{6}{19} \right) = 721/228$.

$$\frac{1}{6} = 3.166\ 666\ 666\ 666$$

$\frac{6}{19} = 3.157\ 894\ 736\ 842 \dots$ recurring after 18 digits.

$$2) 6.3\bar{24}\ 561\ 403\ 508\ 771\ 929\ 82 \dots$$

recurring.

$$3.162\ 2\bar{80}\ 701\ 754\ 385\ 964\ 9\bar{1} \dots$$

1st mean.

$$\frac{2280}{721} = 3.162\ 2\bar{74}\ 618\ 585\ 298\ 196\ 94 \dots$$

1st harmonic mean.

$$2) 6.324\ 555\ 320\ 339\ 684\ 161\ 86 \dots$$

3.162 2 77 660 169 842 080 930 481 543 664 9 ... 2nd mean.

$$\frac{32877160}{1039681} = 3.162\ 2\bar{77}\ 660\ 16\bar{6}\ 916\ 583\ 067\ 306\ 221\ 812\ 2 \dots$$

$$2) 6.324\ 555\ 320\ 336\ 758\ 663\ 997\ 787\ 765\ 477\ 2 \dots$$

3.162 2 77 660 16 8 379 331 998 893 882 738 6 ...

$$\frac{21836443209120}{21618731635121} = 3.162\ 2\bar{77}\ 660\ 16\bar{8}\ 379\ 331\ 998\ 893\ 206\ 126\ 8 \dots$$

$$2) 6.324\ 555\ 320\ 336\ 758\ 663\ 997\ 787\ 088\ 865\ 4 \dots$$

3.162 2 77 660 16 8 379 331 998 893 544 432 7 ...

and so on for about 51 correct digits.

Thus, it is easier to take the mean of the actual quotients of $\frac{1}{6}$ and $\frac{6}{19}$ than to divide 721 by 228, because not only are two narrow division sums easier than one broad one, but use may be made of the two recurring quotients to write down as many digits as will be wanted subsequently. It will be noted that vulgar fractions are used in preference to decimal notation until an actual division is required to give a result in usual form. If decimal

fractions had been used they would all have had to be expressed with an accuracy equal to that of the ultimately intended result. The multiplication and division sums would then have been as broad as they are long and all as long as the last, and this is as objectionable in such sums as it is in a story. The vulgar fractions are absolute values, and so the division sums, however long, may be as narrow as the numerator allows.

One feature of the method is the extreme rapidity of the convergence, and this becomes rapidly greater as the first approximation is nearer to the true root. Thus the number of significant zeros—that is, including the one before the decimal point—in the errors of the third mean with $3\frac{1}{4}$, $3\frac{1}{5}$, $3\frac{1}{6}$, $3\frac{1}{7}$ as the first approximations are respectively 13, 17, 25 and 19.

It would at first seem that this method of finding square roots is not suitable for finding the root of any long or incommensurable number such as π . As a fact it is available in such cases provided only a vulgar fraction which is a close approximation to the root can be found directly.

This is not so much a school exercise as a practical method of finding a square root with extreme accuracy should anyone happen to require it.

To find the approximate root of a quantity expressed as a vulgar fraction, we use a slide rule of usual construction, the upper lines on the rule and slide being what used to be called *A* lines while the lower lines on each are *D* lines; that is, they are on double the scale of the *A* lines. If then the number is itself a vulgar fraction, we set its denominator on the scale against the numerator on the rule, on the upper lines. We then look along the lower lines for two divisions which are opposite to one another. If these divisions represent single digits, so much the better, but if they represent figures of two digits the process is not thereby unduly lengthened. If the number to be treated is a simple number then we consider the denominator to be 1, or if the number is expressed as a reciprocal, then the numerator of the fraction is 1, and there is no difference in the operation. Any one not well accustomed to a slide rule must beware of making his setting at the wrong half of the rule, whereby he would introduce an error of $\sqrt{10}$ as a factor in the result.

Taking π as an example and setting 1 on the slide against π on the rule, 22 will be found on the slide below against 39 on the rule, so that $39/22$ is a pretty close approximation to $\sqrt{\pi}$. Now multiplying π by 22 and dividing the product by 39 we obtain 1.772 180 471 ..., and taking the arithmetic mean of this and of $39/22 = 1.772$ we obtain 1.772 453 872 0 ...; the last three figures should be 509, so that the first mean is a little over 1 in 100 000 000 too much.

The advantage of a really good first approximation is so great that it is worth while to allow two digits in both numerator and denominator. It is fairly safe to say that for any number whatever a good opposition of divisions can be found in which each represents two digits only. For instance, $3\frac{1}{2}$ was used on account of its ease

and simplicity when illustrating the method on $\sqrt{10}$. Had the slide rule been used, $98/31$ would have been found, and the first arithmetical mean, which could be found on half a postcard, would have been only 1 in 20 000 000 in error. The slide rule shows $19/6$ to be less accurate, but better than $22/7$ or $25/8$.

The use of a slide rule for finding the first approximation might seem to suggest that a three-figure logarithm table—which in effect it is—can be used to find numbers to a large number of places. This fallacy, which was perpetrated by the famous Dr. Hutton, I showed up in the *Gazette* for March 1931. The case, however, is quite different, for Dr. Hutton had no method of rapidly converging approximation.

If a cube root is required the same method may be used, as also with higher roots, but naturally the process becomes the more cumbersome the higher the root.

To find $\sqrt[3]{10}$. By trial $(2\frac{1}{6})^3$ is found to be slightly more than 10, and $(2\frac{1}{7})^3$ less, so take $2\frac{2}{13}$ as an intermediate and nearer value.

$$2\frac{2}{13} = \frac{28}{13}; \quad (\frac{28}{13})^2 = \frac{784}{169}; \quad 10 \div \frac{784}{169} = \frac{1690}{784}.$$

The first mean is :

$$\frac{1}{3}(2 \cdot \frac{28}{13} + \frac{1690}{784}) = \frac{65874}{30876} = 2.15443486.$$

since

$$\sqrt[3]{10} = 2.15443467,$$

the error is less than 1 in 10 000 000 in the first mean.

C. V. Boys.

854. Golf Scores.

<i>A</i>	4	5	5	6	9	4	5	4	4	—46
<i>B</i>	5	6	6	7	3	5	6	3	5	—46
<i>C</i>	6	7	7	8	6	3	3	3	3	—46

A beats *B* by 4 and 3, *B* beats *C* by 5 and 4, *C* beats *A* by one hole. These figures were sent to *The Scotsman*, and a correspondent (J. C. Smith) suggested the following series for three holes.

<i>A</i>	1	2	3	—6	<i>A</i> one up on <i>B</i> .
<i>B</i>	2	3	1	—6	<i>B</i> one up on <i>C</i> .
<i>C</i>	3	1	2	—6	<i>C</i> one up on <i>A</i> .

Similar results can be obtained for four men playing four holes, and so on. [Per Mr. J. W. Stewart.]

855. A "Cubicle" Universe.—From the wrapper of *Proc. Edinburgh Math. Soc.*, ser. 2, vol. ii, part iii. [Per Prof. E. H. Neville.]

856. The death of Archimedes by the hands of a Roman soldier is symbolical of a world-change of the first magnitude. The theoretical Greeks with their love of abstract science, were superseded in the leadership of the European world by the practical Romans. Lord Beaconsfield, in one of his novels, has defined a practical man as a man who practises the errors of his forefathers. The Romans were a great race, but they were cursed with the sterility which waits upon practicality. They did not improve upon the knowledge of their forefathers, and all their advances were confined to the minor technical details of engineering. They were not dreamers enough to arrive at new points of view which could give a more fundamental control over the forces of nature. No Roman lost his life because he was absorbed in the contemplation of a mathematical diagram.—A. N. Whitehead, *An Introduction to Mathematics*. [Per Mr. F. Fox.]

DASHBOARD MATHEMATICS.

By T. C. J. ELLIOTT.

ON the dashboard of a car we have a collection of direct-reading instruments, such as voltmeters, clocks, etc. The great simplification of practical physics which has been brought about by the ever-growing use of direct-reading instruments has unfortunately not been accompanied by an equal simplification of mathematical teaching. Schoolmasters ignore the dashboard, and base their teaching on Greek mathematics, for, though they profess to build on those foundations which are familiar to their pupils, they show a strange partiality for the familiar knowledge of two thousand years ago.

The language and ideas which the dashboard illustrates ought to be as familiar as the dashboard itself and be the foundations of our mathematics. To promote this familiarity, the mathematical classroom should be equipped with an artificial dashboard, the dials of which can be linked in various ways at will by unseen mechanism behind the board. An outline of the use of such an instrument was attempted in my book on the so-called *Dial Machine* (Peterborough Press, 1926).

The pupil should be told that the subject he is to study is the dependence and independence of dial readings, and whether this subject is called Algebra, or "The Geometry of N Dimensions", seems to be a matter of indifference. If we call it algebra, then the readings of a dial are called a *variable*, and, if we call it geometry, then the readings are called a *dimension*. The learned name for a linkage or dependence between dials is "function", and that for an independence, or freedom of dials from each other, is "space", the former word coming from algebra, the latter from geometry. The subject is really a *descendant* from the old algebra and geometry, and might perhaps be known in schools by a combined word, such as "algeometry" (note by the writer in *Math. Gazette*, Dec. 1929).

The word "board" may be used both for linkages and spaces, that is to say, we can use it for a set of dials without implying anything as to their relative linkage or freedom. Other words which are used both for linkages and spaces are the terms "point" and "at right angles". Any association of readings is called a "point"; for example, if we reduce the board to only two dials, then any two readings of them form a point, and all possible points a "space of two dimensions". Thus the dashboard points can be classed as points of one dimension, points of two dimensions, and so on. This of course seems very different from Euclid's definition of a point, but then Euclid was not familiar with the dashboard, and the modern schoolboy is.

Our meaning of "at right angles" is the one which presents most difficulty to the beginner, whose mind has been saturated with the ancient definitions, because, on the dashboard, its use is entirely a matter of definition. We simply *define* all variables to be "at right angles", and therefore disregard entirely the evidence of the carpenter's setsquare. We adopt for describing dependence and

independence of variables the language which *would be used* if the dependence were to be graphed on squared paper, but we employ this language *irrespective of whether it is graphed or not*, and therefore we employ the phrase "at right angles" irrespective of whether the dependence is graphed or not. To prevent confusion with the Euclidean right angle, we might perhaps reserve a workshop term, such as "square" angle, for the latter. Or again, we might leave the word "right angle" for use in physical geometry, and employ "cross angle" instead for the dial machine, a usage suggested by the expression "classification and cross-classification".

There is a similar difficulty in the case of words like "line", which may perhaps be overcome by prefixing the word "dial". For example, we might call a one-to-one linkage or correspondence between two dials a *dial line in a dial space of two dimensions*, and a one-to-one linkage between three dials a *dial line in a dial space of three dimensions*, and so on. Or we might use words like "line" without any prefix for the dial machine, but prefix the word "folk" to show their Euclidean use, as is suggested by phrases like "folk songs" or "folk dancing".

Next to the dashboard meaning of "right angle", perhaps the most serious difficulty arises from the general ignorance of linkages higher than one-to-one. It is easy to find elementary illustrations of these last; for example, any transitive verb is a correspondence of "subject" to "object"; an English-French dictionary gives a correspondence of English words to French words; or we can instance the correspondence of a boy's height to his age. But, if we venture to call a surface a "one-to-two" correspondence, and if we go on to speak of "one-to-three" and "one-to-four" correspondences, a thick fog descends, which perhaps can be dissipated only by actually showing examples of such linkages between dials.

The paucity of suitable words may, to some extent, be remedied by a simple notation suggested by assigning a capital letter to each dial, if free, and using the corresponding small letter for a constant. For example, we can show a space by writing the letters concerned together, and a linkage by enclosing the letters in a bracket. Thus ABC may be taken as a *statement* that the three dials A, B, C , form a space, and (ABC) as a statement that they form a "surface" in that space. The particular kind of surface is supposed to be shown by the shape of the bracket, so that $\{ABC\}$ is a different surface from (ABC) . To write two brackets together is to imply the existence of two superimposed conditions or mechanisms behind the board. It will be seen that our bracket plays the part of a primitive kind of equation, though there is no sign of equality, no plus or minus sign, no indices, etc., and we use capital letters in place of x and y .

The dashboard meaning of "space", "line", "surface", etc., may perhaps be conveniently recalled by the words *Liberty, Equality, Fraternity*. A liberty or freedom of dials from each other is a "space"; an equality of readings on two dials is one of the simplest and most important functions or "lines"; and a fraternity of dials

is a dependence such that locking or clamping a certain number locks all the rest, for example, in the case of a "surface", to lock any two of the dials concerned locks all the rest.

It may also be of assistance to use names of quantities in the place of letters, for example to show the dependence of age on height by *writing* the words "age", "height", together in a bracket. Or again, we might contrast the dashboard definitions with those of Euclid by labelling four dials length, breadth, thickness, fourthness. For example, a "line" is not "that which has length without breadth", but it is "that which has length *dependent on breadth*"; a "surface" is not "that which has length and breadth but no thickness", but it is "that which has length *dependent on breadth as well as on thickness*". In other words, when length is given, then we have a line dependence of breadth on thickness, or (in the case of a space of four dimensions) a line dependence of breadth on thickness and fourthness.

Of course at the present day it is the conflict between new and old which perplexes the beginner, the fact that he has not only to learn but to unlearn, or at least arrange his knowledge in a fresh order, but one may hope that, when the advantages of "Algeometry" are more widely understood, it will hold the field alone for beginners, and that they will approach Cartesian geometry and Euclid through it instead of following the reverse order, the method suggested being that of using a net or lattice as a picture of the dashboard.

If we suppose the dial scales to be uncurled, and so arranged as to visualise the language of the dial machine, a space in the above sense is pictured by a net or lattice having the uncurled scales as its threads. It is true that this method limits us to spaces of not more than three dimensions, but the net may be as irregular as we please without at all impairing the correctness of the language drawn from the dial machine. Therefore, starting from the use of the dial machine, the source from which we suppose the language of the beginner to be taught, our first contact with the common school geometry is in Cartesian geometry, in which a special meaning is given to the words "straight", "equal", and "right angle", though so far as the language of the dial machine is concerned, one kind of net is as rectangular and equally spaced as another.

The next step, that from Cartesian geometry to the familiar region of euclidean geometry, is simple provided we take Euclid's view that it is possible to attach meaning to the word "point" without any use of coordinates, a view, however, which seems to imply that we know absolute position instead of merely relative position. Two of Euclid's points determine the physical straight line joining them, and Euclid's "equal" fixes his meaning of "right angle", so that we seem to be able to dispense with the use of axes of coordinates entirely, though only at the cost of an intrusion of physics into pure mathematics, that is, into the theory of the dashboard of pure dials. The euclidean method was natural and excusable at a time when the physics in question passed as familiar knowledge, its difficulties being ignored, and when the chariots were not fitted with dashboards.

The above reversal of the customary order of teaching is more likely to be brought about by science teachers than by mathematical teachers, for the latter are becoming fossilized in the same way as teachers of the classics, who began by being science teachers, but got left behind in the march of progress, so that their work had to be taken over by a new class of teachers. The flood of books with such titles as "Mathematics for the Engineer", "Mathematics for the Chemist", and so forth, is perhaps a sign of what is happening, for the authors in many cases are really trying to build on modern foundations as opposed to Greek ones, and particularly on the everyday illustrations of the function concept. This aim is generally misrepresented by the teacher of classical mathematics, just as the aims of the science teacher are misrepresented by the old-fashioned teacher of Greek and Latin.

Among the illustrations of the function concept should be included the numbers themselves, for the unifying influence of the dashboard should be extended even to the most elementary treatment of Arithmetic. The characteristic of a one-to-one function is that it can be tabulated in a double column, but, if we happen to have a name for the function itself, then we need names for the things in only one of the columns, because the other column can be filled in with functions of those things. For that reason the fact that any number, such as three, is a function, and can be tabulated in a double column, is often overlooked. In the infant school it should be pictured by a double column showing one apple or orange or dog on one side, and three apples or oranges or dogs on the other, and an English-French dictionary should be pictured in the same way to show the resemblance, and it should be explained that words like direct and inverse, product, factor, etc., apply as much in the one case as the other. For example, an English-German dictionary is the product of an English-French by a French-German, just as six is the product of three by two; a French-English dictionary is the inverse of an English-French, just as the function "a sixth" is the inverse of six; and the product of an English-French by a French-English is the function "one", just as the product of six by "a sixth" is the function "one".

An early acquaintance with the idea of function also has the advantage that it is now possible to refer to functions as things to be counted, and therefore to the number of factors in a product of functions, and therefore to the idea of power and of index showing the number of times a function has been multiplied by itself. It may indeed be argued that we ought to explain a dial reading as a power of the unit step or division on the dial, and explain addition in general as fundamentally an addition of indices, instead of making the addition of indices a comparatively late stage of instruction.

T. C. J. ELLIOTT.

857. There is no law of nature to the effect that what is taught at school must be true. B. Russell, *A.B.C. of Relativity*, p. 167. [Per Mr. C. W. Adams.]

DETERMINATION OF THE FOCI, DIRECTRICES AND AXES OF A CONIC WHOSE EQUATION IS GIVEN WITH NUMERICAL COEFFICIENTS.

By K. D. PANDAY.

1. The foci, directrices and the eccentricity of the conic given by the general equation of the second degree can be determined by the use of the fundamental property of the conic, that $SP = e \cdot PM$. Let the equation be

$$ax^2 + 2hxy + by^2 + 2gx + 2fy + c = 0.$$

Write this in the form

$$\lambda x^2 + 2gx + \lambda y^2 + 2fy + c = (\lambda - a)x^2 - 2hxy + (\lambda - b)y^2. \dots \dots \dots \text{(i)}$$

Choose λ so that the right-hand member is a perfect square, so that

$$(\lambda - a)(\lambda - b) = h^2; \dots \dots \dots \text{(ii)}$$

we have two values of λ , say, λ_1, λ_2 , both real. Let λ_1 be greater than a , λ_2 less than b . Then (i) becomes

$$\lambda x^2 + 2gx + \lambda y^2 + 2fy + c = \{(\lambda - a)^{\frac{1}{2}}x \pm (\lambda - b)^{\frac{1}{2}}y\}^2,$$

the upper or lower sign being taken as h is negative or positive.

We may write this in the form

$$\begin{aligned} \lambda x^2 + 2x\{g + k(\lambda - a)^{\frac{1}{2}}\} + \lambda y^2 + 2y\{f \pm k(\lambda - b)^{\frac{1}{2}}\} + c + k^2 \\ = \{(\lambda - a)^{\frac{1}{2}}x \pm (\lambda - b)^{\frac{1}{2}}y + k\}^2. \end{aligned}$$

Then (α, β) will be a focus of the conic if

$$\alpha = -\{g + k(\lambda - a)^{\frac{1}{2}}\}/\lambda, \beta = -\{f \pm k(\lambda - b)^{\frac{1}{2}}\}/\lambda, a^2 + \beta^2 = (c + k^2)/\lambda, \dots \text{(iii)}$$

provided the upper or lower sign in the value of β is taken according as h is negative or positive.

The directrix is the line

$$(\lambda - a)^{\frac{1}{2}}x \pm (\lambda - b)^{\frac{1}{2}}y + k = 0, \dots \dots \dots \text{(iv)}$$

and the eccentricity is $(2\lambda - a - b)^{\frac{1}{2}}/\lambda$. The curve is an ellipse, parabola or hyperbola according $\lambda \leq (a + b)$.

The equation for k is found to be

$$k^2(\lambda - a - b) + 2k\{g(\lambda - a)^{\frac{1}{2}} \pm f(\lambda - b)^{\frac{1}{2}}\} + g^2 + f^2 - c\lambda = 0, \dots \text{(v)}$$

by eliminating α, β from equations (iii) above, and keeping the convention with regard to the ambiguity of sign. We shall see in section 3, below, the rule for the value of λ which will give the real foci and directrices of the central conics.

2. The case of the parabola presents the least difficulty. We have $\lambda = a + b$, and thus

$$2k\{g\sqrt{b} \pm f\sqrt{a}\} + g^2 + f^2 - c(a + b) = 0,$$

$$\text{and } a = -(g + k\sqrt{b})/(a + b), \beta = -(f \pm k\sqrt{a})/(a + b).$$

The value of λ gives the equation to the directrix in the form

$$x\sqrt{b} \pm y\sqrt{a} + k = 0.$$

3. A consideration of equation (v) shows that the discriminant of the k equation is

$$4[g^2(\lambda - a) + f^2(\lambda - b) - 2fgh - (\lambda - a - b)(g^2 + f^2 - c\lambda)],$$

reducing to $4[af^2 + bg^2 + ch^2 - abc - 2fgh]$.

This is enough to show that, in order to get the real foci and directrices, λ should be chosen so that

$$\lambda - a, \quad \lambda - b, \quad \text{and} \quad af^2 + bg^2 + ch^2 - abc - 2fgh$$

have the same sign.

For example, in the curve

$$3x^2 + 8xy - 3y^2 - 40x - 20y + 50 = 0,$$

the values of λ are ± 5 , and

$$af^2 + bg^2 + ch^2 - abc - 2fgh = -1250.$$

Thus the negative value of λ will give the real foci and directrices.

4. The real utility of these results, especially of equation (iii), is observed in the determination of the axes of the conic.

Eliminating k from the equations

$$a = -\{g + k(\lambda - a)^{\frac{1}{2}}\}/\lambda, \quad \beta = -\{f \pm k(\lambda - b)^{\frac{1}{2}}\}/\lambda,$$

or from the equations

$$a + g/\lambda = -k(\lambda - a)^{\frac{1}{2}}/\lambda, \quad \beta + f/\lambda = \pm k(\lambda - b)^{\frac{1}{2}}/\lambda,$$

we have $(\lambda - b)^{\frac{1}{2}}(a + g/\lambda) \pm (\lambda - a)^{\frac{1}{2}}(\beta + f/\lambda) = 0$;

thus the foci lie upon the straight line

$$(\lambda - b)^{\frac{1}{2}}(x + g/\lambda) \pm (\lambda - a)^{\frac{1}{2}}(y + f/\lambda) = 0, \quad \dots \dots \dots \text{(vi)}$$

which is therefore the equation to the major or to the transverse axes, as the case may be.

The minor or conjugate axis is that on which the imaginary foci lie, and equation (vi) will give that axis if we put for λ the value which does not give the real foci. We can put the equation in the form

$$(\lambda - b)(x + g/\lambda) - h(y + f/\lambda) = 0,$$

since $(\lambda - a)^{\frac{1}{2}}(\lambda - b)^{\frac{1}{2}} = -h$.

I have not seen the equations to the axes of the general conic given separately anywhere, although the form here obtained is so simple. Of course the choice of the proper value of λ requires the determination of the sign of $af^2 + bg^2 + ch^2 - abc - 2fgh$.

The method for determining the foci and the directrices is not new, but I know of no text-book that gives the equation to the axes in this form, which is evidently much simpler than the one that is usually presented.

K. D. PANDAY.

A CLASS OF HOMOGENEOUS LINEAR DIFFERENTIAL
EQUATIONS OF THE SECOND ORDER.

BY F. UNDERWOOD, M.Sc.

1. In a recent paper Mammana* considered the equation

$$z'' + pz' + qz = 0, \dots \quad (1)$$

where x is the independent variable and p and q are functions of x , real, finite and continuous in the interval (a, b) , and investigated the possibilities of its decomposition into the form

$$(D + a)(D + \beta)z = 0, \dots \quad (2)$$

where $D \equiv d/dx$ and a and β are functions of x .

If (1) and (2) represent the same equation,

$$a + \beta = p; \quad \beta' + a\beta = q, \dots \quad (3)$$

and eliminating a , we see that β must satisfy the Riccati equation,

$$\beta' = \beta^2 - p\beta - q. \dots \quad (4)$$

If an integral β of (4) can be found which is real in the interval (a, b) , a is given by (3), and the formal expression for the integral of (1) valid in the same interval is

$$z = k \left[\int_a^x \exp \left\{ \int_a^x (\beta - a) dx \right\} dx + h \right] \exp \left(- \int_a^x \beta dx \right). \dots \quad (5)$$

Mammana showed that it is not always possible to decompose (1) into the form (2) if the restriction is made that the functions a and β must be real in the interval (a, b) . Even when such functions exist, the method (considered only from the elementary point of view as a practical method of finding the complete primitive) is liable to fail owing to the difficulty usually encountered in finding an integral β of equation (4). It is proposed to consider here some cases of (1) in which real functions a and β exist and in which the complete primitive (5) can be written down.

2. The first class (or sub-class) consists of equations of the type

$$z'' + Iz = 0, \dots \quad (6)$$

where

$$I = \phi' - \phi^2, \dots \quad (7)$$

ϕ being a function of x which is real, finite and continuous in the interval (a, b) . Forms (1) and (2) reduce to forms (6) and (7) by putting $\beta = -a = \phi$, so that $p = 0$, $q = \phi' - \phi^2$, and the complete primitive takes the form

$$z = k \left[\int_a^x \exp \left(\int_a^x 2\phi dx \right) dx + h \right] \exp \left(- \int_a^x \phi dx \right).$$

* G. Mammana, "Sopra un nuovo metodo di studio delle equazioni differenziali lineari", *Math. Zeit.*, 25 (1926), 734-748. This work is continued and extended to equations of the n th order in a more recent paper, "Decomposizione delle espressioni differenziali lineari omogenee in prodotti di fattori simbolici e applicazione relativa allo studio delle equazioni differenziali lineari", *Math. Zeit.*, 33 (1931), 186-231.

An example of this class is given by (xi)—(a) above, which may be treated either by the method there given for the third class, or may be considered as belonging to the fourth class, where $\psi = \sec x$, $\phi = \tan x$. Similarly (xi)—(b) may be taken as belonging to the fourth class where $\psi = \sec x$, $\phi = -\tan x$.

6. In the case of the general equation (1) treated by Mammana, the real difficulty (as mentioned in § 1) of finding the complete primitive lies in the search for an integral β of equation (4). A few examples in which β may be found readily are given below. They are also interesting because in each case the coefficients of the equation are simply periodic and both the fundamental integrals are periodic.

$$(xii) \quad z'' \cos x \sin^2 x - \sin x (2 \cos^2 x - \sin^2 x) z' + 2z \cos^3 x = 0.$$

Particular integrals of (4) are $-\cot x$ and $-2 \cot x$, so the general value of β , λ being a constant, is

$$\cot x (\lambda - 2 \sin x) / (\sin x - \lambda).$$

$$z = A \sin x + B \sin^2 x.$$

$$(xiii) \quad \cos x (\cos^2 x + 2 \sin^2 x) z'' - \sin x (\cos^2 x - 2 \sin^2 x) z' + 2z \cos^3 x = 0.$$

A particular value of β is $-\cot x$.

$$z = A \sin x + B \cos^2 x.$$

$$(xiv) \quad z'' \cos^2 x \sin^2 x + \cos x \sin x (\cos^2 x - \sin^2 x) z' - z = 0.$$

Particular values of β are $\pm \sec x \operatorname{cosec} x$, and the general value is

$$(\lambda \cos^2 x + \sin^2 x) / \cos x \sin x (\lambda \cos^2 x - \sin^2 x) :$$

$$z = A \tan x + B \cot x.$$

$$(xv) \quad z'' \cos^2 x \sin^2 x - 2z' \cos x \sin x (1 + \sin^2 x) + 2z = 0.$$

Particular values of β are $-\sec x \operatorname{cosec} x$ and $-2 \sec x \operatorname{cosec} x$, and the general value is

$$(\lambda \cos x - 2 \sin x) / \cos x \sin x (\sin x - \lambda \cos x) :$$

$$z = A \tan x + B \tan^2 x.$$

In examples (xii)-(xv) the value of a and the complete primitive are found immediately when any one value of β has been found from equation (4).

It is not claimed, of course, that the method used is necessarily the shortest and most convenient for a particular example, but the equations used here furnish convenient examples of the application of Mammana's method to the classes defined above. I do not know if these classes and methods have been given elsewhere, but they may have some interest, and in all cases the complete primitive is obtained readily by the method indicated for that class of equation.

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F. UNDERWOOD.

858. From *Conway Letters*, by M. H. Nicholson (Yale University Press, 1930). No. 23 (p. 60). John Finch to Anne Conway. December 1/11, 1651.

"I think it be much better to defer your Arithmetique to the spring than to sett upon it alone for that which you gett comes hard nor will your definitions be so good as an experienced master . . ." [Per Mr. F. Puryer White.]

MATHEMATICAL NOTES.

1022. *The nature of the roots of a cubic equation by graphical considerations.*

In Note 996 (*Math. Gazette*, July 1931) Mr. Hinckley gave graphical interpretations of the operations in Theory of Equations in the case of a Cubic, leading to an investigation of the nature of the roots.

The discriminant for the nature of the roots can be obtained, more shortly, as follows :

The graph of $y = x^3 + G$ is that of the familiar x^3 curve moved through a distance G , upwards if G is positive, downwards if G is negative.

Let P be the point on the curve, the tangent at which passes through the origin. We have for this point $\frac{dy}{dx} = \frac{y}{x}$, so the x coordinate of P is given by $3x^2 = x^3 + Gx^{-1}$, i.e. by $x^3 = \frac{3}{2}G$, and the gradient of OP is $3(\frac{1}{2}G)^{\frac{1}{3}}$, which is always positive.

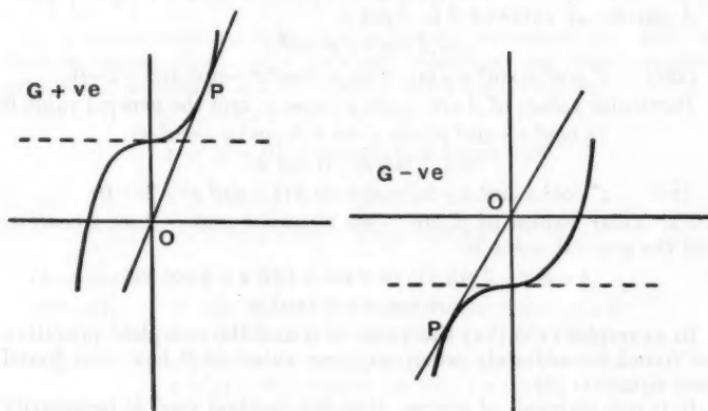


FIG. 1.

FIG. 2.

Consider the intersection of the curve with the straight line $y = H'x$, the gradient of which is H' . There is clearly only one real point of intersection (i) if H' is negative, (ii) if H' is positive and $< 3(\frac{1}{2}G)^{\frac{1}{3}}$, but three real points if $H' > 3(\frac{1}{2}G)^{\frac{1}{3}}$.

Replace H' by $-3H$.

The equation $x^3 + 3Hx + G = 0$ will thus have three real roots only if $-3H > 3(\frac{1}{2}G)^{\frac{1}{3}}$, i.e. if $0 > 4H^3 + G^2$.

So $4H^3 + G^2$ is negative for three real roots and is zero for co-incident roots.

H. E. PIGGOTT.

1023. *The Arithmetic and Geometric Mean.*

The following short proof that the arithmetic mean is greater than the geometric, if the numbers involved are all positive but not all equal, is based on the inequalities

$$e^{\pm x} > 1 \pm x. \quad \dots \quad (\text{A})$$

This is proved by Chrystal, *Algebra*, Vol. II, p. 80, but can be more shortly proved as follows. Let x be positive; then

$$e^x = 1 + x + \frac{1}{2}x^2 + \dots$$

$$> 1 + x.$$

$$e^x < 1 + x + x^2 + \dots \text{ which } = (1 - x)^{-1}, \text{ if } 1 - x > 0.$$

Thus $e^{-x} > 1 - x$, if $1 - x > 0$.

If $1 - x$ is negative, it is obvious that e^{-x} , which is positive, is greater than $1 - x$.

Proceeding to the main proposition, let the numbers be a_1, a_2, \dots, a_n . Then

$$\frac{1}{n} \sum a \geq (a_1 a_2 \dots a_n)^{1/n},$$

$$\text{as } \left(\frac{1}{n} \sum a \right)^n \geq a_1 a_2 \dots a_n,$$

$$\text{i.e. as } \begin{aligned} 1 &\geq (n a_1 / \sum a) (n a_2 / \sum a) \dots (n a_n / \sum a), \\ &\geq (1 + \lambda_1) (1 + \lambda_2) \dots (1 + \lambda_n) \end{aligned}$$

$$\text{where } \lambda_t = (n a_t / \sum a) - 1, \text{ and } \sum \lambda_t = 0.$$

Now by (A), e^λ is greater than $1 + \lambda$, and so (since not all the λ 's can vanish, and if some of them vanish the corresponding factors are unity and do not disturb the inequality)

$$1 = e^0 = e^{\sum \lambda} > (1 + \lambda_1) (1 + \lambda_2) \dots (1 + \lambda_n);$$

and the theorem is proved.

The use of the Exponential Theorem makes the demonstration slightly less elementary than those usually given, but in text-books the Exponential Theorem is usually proved before the subject of Inequalities is treated.

G. J. LIDSTONE.

1024. *A practical approximation to the perimeter of the Ellipse.*

The following formula (1), which does not seem to be very generally known, is given without proof in *A Manual of Mathematics* by Hudson and Lipka [New York, John Wiley & Sons Inc. ; London, Chapman & Hall, Ltd.—a useful compendium of formulae and 4-place tables]. If s is the perimeter of an ellipse of semi-axes a and b ,

$$s = \pi(a + b) \left[1 + \frac{1}{4} \left(\frac{a - b}{a + b} \right)^2 + \frac{1}{8} \left(\frac{a - b}{a + b} \right)^4 + \frac{1}{32} \left(\frac{a - b}{a + b} \right)^6 + \dots \right]. \quad (1)$$

The general term is not given, but is shown below to be

$$\left\{ \frac{1 \cdot 3 \dots (2m - 3)}{2 \cdot 4 \dots (2m - 2)} \frac{1}{2m} \right\}^2 \left(\frac{a - b}{a + b} \right)^{2m}.$$

The series converges more quickly than the usual series in powers of e^2 , where e is the eccentricity, because the coefficients and the powers are both smaller in (1).

Putting* $\lambda = \left(\frac{a-b}{2(a+b)} \right)^2$, (1) may be written

$$s = \pi(a+b)[1 + \lambda + \frac{1}{4}\lambda^2 + \frac{1}{4}\lambda^3 + \dots].$$

It is easily shown that the terms up to λ^3 and a considerable part of the later terms may be represented by the compact expression

$$s \doteq \frac{1}{4}\pi(a+b) \cdot [3(1+\lambda) + 1/(1-\lambda)], \dots \quad (2)$$

which is the proposed practical approximation. This gives a good approximation even for an ellipse of high eccentricity.

Thus, if $e = \sin 80^\circ = 0.9848$, $b/a = \cos 80^\circ = 0.1736$, $s/a = 4.1604$ and (2) gives 4.1602. If $e = \sin 85^\circ = 0.9962$, $b/a = \cos 85^\circ = 0.0872$, $s/a = 4.0506$ and (2) gives 4.0496. If $e \rightarrow 1$, $b/a \rightarrow 0$, $s/a \rightarrow 4$ and (2) $\rightarrow 3.9925a$ giving an error of less than 1 in 500 in this extreme limiting case. For moderate eccentricities (2) gives very close approximations.

It will be seen that the numerical coefficients in (1) are the squares of the coefficients in the expansion of $(1-x)^{\frac{1}{2}}$: thus, putting $(a-b)/(a+b) = u$, the terms in the series are the squares of the coefficients of the powers of $z^{\pm 1}$ in the expansion of $(1-uz^{\pm 1})^{\frac{1}{2}}$; and so the series represents the term independent of z in the expansion of $(1-uz)^{\frac{1}{2}} \cdot (1-u/z)^{\frac{1}{2}}$. This suggests the following simple proof of (1); I have been helped in this proof by Mr. J. M. Whittaker, D.Sc., of Pembroke College, Cambridge. Starting with the known expression

$$s = \int_0^{2\pi} (a^2 \cos^2 \theta + b^2 \sin^2 \theta)^{\frac{1}{2}} d\theta,$$

it is easily shown that the expression under the radical is equal to

$$\begin{aligned} & \frac{1}{2}[a^2 + b^2 + (a^2 - b^2)(\cos^2 \theta - \sin^2 \theta)] \\ &= \frac{1}{4}[(a+b)^2 + (a-b)^2 + 2(a^2 - b^2) \cos 2\theta] \\ &= \left(\frac{a+b}{2}\right)^2 \left[1 + \left(\frac{a-b}{a+b}\right)^2 - 2\frac{a-b}{a+b} \cos 2\theta\right] \\ &= \left(\frac{a+b}{2}\right)^2 \left[1 + u^2 - 2u \cos 2\theta\right]. \end{aligned}$$

Hence $s = \frac{1}{2}(a+b) \int_0^{2\pi} (1 + u^2 - 2u \cos 2\theta)^{\frac{1}{2}} d\theta \dots [u \equiv (a-b)/(a+b)].$

Now if $z \equiv e^{2i\theta}$, $1 + u^2 - 2u \cos 2\theta = (1-uz)(1-u/z)$, and

$$(1 + u^2 - 2u \cos 2\theta)^{\frac{1}{2}} = (1-uz)^{\frac{1}{2}} \cdot (1-u/z)^{\frac{1}{2}}.$$

From the foregoing remarks it follows that the product on the r.h.s. = [the series in (1)] + [terms of the form $C(z^u + z^{-u}) = 2C \cos 2u$].

* It may be noted that if we put $e = \sin \phi$ we have $b/a = \cos \phi$ and $\lambda = \frac{1}{4} \tan^4 \phi$, a very convenient form for logarithmic calculation.

Integrating from 0 to 2π , terms of the form $\int_0^{2\pi} 2C \cos 2u \theta \cdot d\theta$ vanish, and so we have

$$s = \frac{1}{2}(a+b) \int_0^{2\pi} [\text{the series in (1)}] d\theta$$

$= \pi(a+b)$ [the series in (1)]. Q.E.D.

The general term in the series (1) is the square of the coefficient of z^m in the expansion of $(1 - uz)^{\frac{1}{2}}$, that is,

$$\left\{ \frac{1 \cdot 3 \cdot \dots \cdot (2m-3)}{2 \cdot 4 \cdot \dots \cdot 2m} \right\}^2 u^{2m}.$$

It is not hard to find a simple expression for an upper limit of the error involved in the approximate formula. It will be found that the approximate value is less than the true value by

$$\pi(a+b)\left[\frac{9}{8}\lambda^4 + \frac{33}{8}\lambda^5 + \frac{377}{38}\lambda^6 + \dots\right]$$

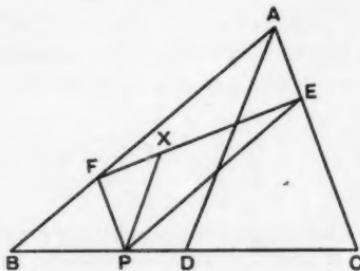
where the numerical coefficients are increasing in a varying ratio always less than 4. Thus the sum of the series [] is less than $\frac{9}{\pi} \lambda^4 / (1 - 4\lambda)$; and so the error in the perimeter is *greater* than $\frac{9}{\pi} \lambda^4 \cdot \pi(a+b)$ and *less* than $\frac{9}{\pi} \lambda^4 \cdot \pi(a+b) / (1 - 4\lambda)$. Also, since both the true and approximate values are greater than $\pi(a+b)(1+\lambda)$, the

the true and approximate values are greater than $(1 + \lambda)(1 + \lambda)$, the proportionate error is less than $\frac{9}{64} \cdot \frac{\lambda^4}{(1 - 4\lambda)(1 + \lambda)}$ times either the true or the approximate value. The upper limit of the error is very near to the true error for ellipses of moderate eccentricity.

G. J. LIDSTONE.

1025. The Parabola $a^2a^2 = 4bc\beta\gamma$.

If P is any point on the side BC of a triangle ABC , and if PE, PF be drawn parallel to BA, CA respectively, then EF envelopes the parabola $a^2a^2 = 4bc\beta\gamma$ and touches its envelope at the point in which the line through P parallel to the median AD meets EF .



Let $BP = x$, then the equation to EF is

that is, $x^2(aa + b\beta + c\gamma) - xa(aa + 2c\gamma) + a^2c\gamma = 0$,
the envelope of which is

$$S \equiv a^2 a^2 - 4bc\beta\gamma = 0.$$

Now (i) is the tangent to $S=0$ at the point X with coordinates
 $(2x(a-x)/a, (a-x)^2/b, x^2/c)$,

and the equation to PX is

$$(2x-a)aa+2xb\beta-2(a-x)xc\gamma=0$$

$$\text{or } (2x-a)(aa+b\beta+c\gamma)+a(b\beta-c\gamma)=0,$$

and hence PX is parallel to the median AD .

The equation of the circle AEF is

$$\{x(c\beta-b\gamma)+ab\gamma\}(aa+b\beta+c\gamma)-a^2(a\beta\gamma+b\gamma a+c\alpha\beta)=0,$$

which is satisfied for all values of x by the ratios

$$2bc \cos A : ab : ca ;$$

hence the focus of the parabola is known ; if K be the symmedian point of ABC , then the focus is the point of intersection of AK with the Brocard circle of ABC .

The minimum value of EF is Δ/AD , where Δ is the area of the triangle ABC .

E. G. Hogg.

1026. *The angle between the lines in which a quadric cone is cut by a plane through the vertex.*

A very familiar piece of bookwork is that which obtains the angle between the lines in which the quadric cone

$$ax^2+by^2+cz^2+2fyz+2gzx+2hxy=0$$

is cut by the plane through its vertex,

$$ux+vy+wz=0 ;$$

yet the proof given in the text-books* is not altogether satisfactory.

It is proved that if $(l_1, m_1, n_1)(l_2, m_2, n_2)$ are the direction cosines of the lines in question, then

$$\frac{l_1 l_2}{bw^2+cv^2-2fvw} = \frac{m_1 m_2}{cu^2+aw^2-2gwu} = \frac{l_1 m_2 + l_2 m_1}{-2(hw^2+cuv-fuw-gvw)} \\ = \frac{l_1 m_2 - l_2 m_1}{\pm 2\{(hw^2+\dots)^2 - (bw^2+\dots)(cu^2+\dots)\}^{\frac{1}{2}}} \dots \dots \dots \text{(i)}$$

and the reader is left to verify that after cancelling and collecting terms, the last-given denominator is equal to $\pm 2w\Delta^{\frac{1}{2}}$ where

$$\Delta = \begin{vmatrix} a & h & g & u \\ h & b & f & v \\ g & f & c & w \\ u & v & w & 0 \end{vmatrix}.$$

It seems worth while to give the underlying reason for this result. The denominator of the first of the fractions (i) can be written $-v(fw-cv)+w(bw-fv)$; and this is evidently

$$- \begin{vmatrix} b & f & v \\ f & c & w \\ v & w & 0 \end{vmatrix} = -A,$$

* E.g. Bell, *Coordinate Geometry of Three Dimensions*, p. 90.

where A is the co-factor of a in Δ . Similarly, the denominator of the second of these fractions is $-B$ and that of the third is $-2H$. Hence the result (i) can be written

$$\frac{l_1 l_2}{A} = \frac{m_1 m_2}{B} = \frac{l_1 m_2 + l_2 m_1}{2H} = \frac{l_1 m_2 - l_2 m_1}{\pm 2(H^2 - AB)^{\frac{1}{2}}}. \quad \text{(ii)}$$

Now consider the adjoint determinant of Δ , namely,

$$\begin{vmatrix} A & H & G & U \\ H & B & F & V \\ G & F & C & W \\ U & V & W & D \end{vmatrix}.$$

It is a particular case of a well-known theorem that any two-rowed minor of this determinant is equal to the product of Δ by the algebraic complement of the corresponding minor of Δ . Hence

$$\begin{vmatrix} A & H \\ H & B \end{vmatrix} = -w^2 \Delta.$$

So that (ii) becomes

$$\frac{l_1 l_2}{A} = \frac{m_1 m_2}{B} = \frac{l_1 m_2 - l_2 m_1}{\pm 2w \Delta^{\frac{1}{2}}}.$$

R. J. LYONS.

1027. *The operation $\{f(D)\}^{-1} e^{mx}$.*

Mr. F. Underwood * and Prof. J. P. Dalton † have recently discussed the application of Forsyth's expansion theorem. I recently came across an example which is allied to those discussed by these writers.

A class had to find the particular integral of the differential equation,

$$(D^2 + 2D - 3)y = \sinh x,$$

which many did as follows :

$$\begin{aligned} y &= (D^2 + 2D - 3)^{-1} \sinh x \\ &= (1 + 2D - 3)^{-1} \sinh x \quad (\text{putting } D^2 = 1^2) \\ &= \frac{1}{4}(D - 1)^{-1} e^x - \frac{1}{4}(D + 1)^{-1} e^{-x} \\ &= \frac{1}{4}x e^x + \frac{1}{8} e^{-x}, \end{aligned}$$

which is wrong, although a similar method is quite correct with $\sin x$ in place of $\sinh x$. The error occurs in putting $D^2 = 1^2$. It can be shown that, if

$$f(D) = \phi(D) + \psi(D),$$

and if

$$f(m) = 0, \quad \phi'(m) \neq 0,$$

then

$$\{f(D)\}^{-1} e^{mx} \neq \{\phi(m) + \psi(D)\}^{-1} e^{mx}.$$

Thus

$$\{f(D)\}^{-1} e^{mx} = e^{mx} \{f(D + m)\}^{-1} \cdot 1$$

$$= e^{mx} \{D f'(m)\}^{-1} \cdot 1$$

$$= x e^{mx} / f'(m). \quad \text{(i)}$$

* F. Underwood, *Math. Gazette*, XV, 99 (1930).

† J. P. Dalton, *Math. Gazette*, XV, 369 (1931).

$$\begin{aligned} \text{But } \{ \phi(m) + \psi(D) \}^{-1} e^{mx} &= e^{mx} \{ \phi(m) + \psi(D+m) \}^{-1} \cdot 1 \\ &= e^{mx} \{ D\psi'(m) \}^{-1} \cdot 1 \\ &= x e^{mx} / \psi'(m). \quad \dots \dots \dots \text{(ii)} \end{aligned}$$

But (i) and (ii) are not equal unless $\phi'(m) = 0$.

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H. V. LOWRY.

1028. *A useful Lemma.*

If x, y, a, b are positive numbers, x, y each less than or equal to 1,

$$\begin{aligned} (x-a)(y-b) &> xy - ay - bx \\ &> xy - a - b. \end{aligned}$$

It follows that, if $P_1, P_2, P_3 \dots$ are positive numbers each less than or equal to 1, and u_1, u_2, u_3, \dots are positive numbers each less than the corresponding P ,

$$\begin{aligned} (P_1 - u_1)(P_2 - u_2) &> P_1 P_2 - u_1 - u_2, \\ (P_1 - u_1)(P_2 - u_2)(P_3 - u_3) &> (P_1 P_2 - u_1 + u_2)(P_3 - u_3) \\ &> P_1 P_2 P_3 - u_1 - u_2 - u_3, \end{aligned}$$

and so on, to any number of factors.

The simplest case is where the P 's are each equal to unity, and we have

$$(1 - u_1)(1 - u_2)(1 - u_3) \dots > 1 - u_1 - u_2 - u_3 \dots .$$

1. As an application of this special case, we may write

$$S_n = 1 + \frac{1}{1!} + \frac{1}{2!} + \dots + \frac{1}{n!},$$

and prove that

$$\lim_{n \rightarrow \infty} (1 + 1/n)^n = \lim_{n \rightarrow \infty} S_n.$$

We assume n integral :

$$\begin{aligned} (1 + 1/n)^n &= \sum_{m=0}^n (1 - 1/n)(1 - 2/n) \dots (1 - \overline{m-1}/n)/m! \quad \dots \dots \text{(i)} \\ &> \Sigma \{ 1 - m(m-1)/2n \} / m! \text{ (by the lemma)} \\ &> S_n - \frac{1}{2n} S_{n-2} \\ &> S_n \left(1 - \frac{1}{2n} \right). \end{aligned}$$

But it is obvious from (i) that $(1 + 1/n)^n < S_n$. Thus

$$S_n \left(1 - \frac{1}{2n} \right) < (1 + 1/n)^n < S_n.$$

Making n tend to infinity, we have the existence of the limit and its equality with $\lim S_n$.

2. We may use the more general lemma to give an elementary proof of the factor expression for $\sin \alpha$. Assuming that, if x be an

acute angle, $\tan x > x > \sin x$, we see that if x, y are both acute and $x < y$,

$$\begin{aligned}\sin^2 x / \sin^2 y - x^2 / y^2 &= (y^2 \sin^2 x - x^2 \sin^2 y) / y^2 \sin^2 y \\ &< (y^2 x^2 - x^2 y^2 \cos^2 y) / y^2 \sin^2 y\end{aligned}$$

(since $\sin^2 x < x^2$, $\sin^2 y > y^2 \cos^2 y$);

i.e. $\sin^2 x / \sin^2 y - x^2 / y^2 < x^2$.

Since $\sin x / x > \sin y / y$,

$$\sin^2 x / \sin^2 y - x^2 / y^2 > 0.$$

Taking a , in the first instance, to be an angle less than π , and taking

$$\begin{aligned}P_r &\equiv 1 - a^2 / r^2 \pi^2 \equiv 1 - \frac{a^2}{n^2} / \frac{r^2 \pi^2}{n^2}, \\ 1 - \frac{\sin^2 a / n}{\sin^2 r \pi / n} &\equiv P_r - u_r,\end{aligned}$$

where $u_r < a^2 / n^2$, by the proposition just proved. But, with these values,

$$\frac{\sin a}{n \sin a / n} = (P_1 - u_1)(P_2 - u_2) \dots [\cos a / n],$$

and so lies between $P_1 P_2 \dots$ and $P_1 P_2 \dots - u_1 - u_2 \dots$ by the lemma : where the number of u 's is less than $\frac{1}{2}n$.

Each u is less than a^2 / n^2 : hence

$$P_1 P_2 \dots - a^2 / 2n < \frac{\sin a}{n \sin a / n} < P_1 P_2 \dots$$

Taking the limit as n tends to infinity,

$$\frac{\sin a}{a} = \lim P_1 P_2 \dots$$

That is, $\sin a = a(1 - a^2 / \pi^2)(1 - a^2 / 2^2 \pi^2) \dots$ to infinity.

If a is greater than π , but finite and less than $m\pi$, say, the relation between P_r and $P_r - u_r$ will hold for factors beyond the m th, if n is large enough ; and, multiplying by the limits of the factors which precede, the result still holds.

PERCY J. HEWOOD.

1029. Solution of a Differential Equation.

The equation $9xy^2 \frac{d^2y}{dx^2} + 2 = 0$

occurs in A. R. Forsyth's *Treatise on Differential Equations*, but it is one of the few equations to which he does not supply a complete solution ; he obtains two special solutions only.*

Rewriting the equation and using the integrating factor

$$(xdy/dx - y),$$

we get $9x \frac{d^2y}{dx^2} \left(x \frac{dy}{dx} - y \right) = -2 \left(x \frac{dy}{dx} - y \right) / y^2$,

* A. R. Forsyth, *Treatise on Differential Equations*, 4th Ed. (1914), p. 569, General Examples, No. 51 (ii) ; *Solutions of the Examples* . . . , p. 220.

which integrates as

Putting $v = y/x$, the complete primitive can be obtained in the forms :

$$A - 2x^{-1} = 3C^{-\frac{1}{2}} \left\{ (v^2 + C^{-1}v)^{\frac{1}{2}} - \frac{1}{2}C^{-1} \log [v + \frac{1}{2}C^{-1} + (v^2 + C^{-1}v)^{\frac{1}{2}}] \right\},$$

$$A - 2x^{-1} = -3(-C)^{-\frac{1}{2}}\{(-v^2 - C^{-1}v)^{\frac{1}{2}} + \frac{1}{2}C^{-1} \arcsin(-2Cv - 1)\},$$

according as C is positive or negative.

If in (i) we put $C=0$ and integrate we obtain as a special form of the primitive the equation

$$\bar{y}^3 = x(Bx - 1)^2,$$

which includes the two integrals given by Forsyth, which are

$$y^{\frac{1}{2}} - ax^{\frac{1}{2}} = x^{\frac{1}{2}}$$

$$y^{\frac{3}{2}} - bx^{\frac{3}{2}} = -x^{\frac{1}{2}}.$$

H. J. CURNOW.

1030. The Number of Homogeneous Products of the r th Degree in n Letters.

Let this number be denoted by ${}_nH_r$. It is well known that

$$n^H r = n+r-1 C_r,$$

but of the two methods given in books, one requires the Binomial Theorem for a negative index and the other is based on a very tricky piece of mathematical induction. The following method gives the result directly.

Let the n letters be a, b, c, \dots, k ; and suppose them to be written down in some definite order. Now let the various combinations, r at a time, of the $n+r-1$ quantities

$a, b, c, \dots k ; 2, 3, 4 \dots r ;$

be written down, according to the following rules.

- (i) In any combination in which numbers appear, the numbers are to be placed in the position specified by their values.
- (ii) Subject to the limitation imposed by rule (i), the letters are to appear in the order stated.

Thus we have such arrangements as follow:

abc45e7k, c234dfk8, ad3g5678, etc.

Now in each of these, let each number which appears, be replaced by the letter that it follows ; thus in the first of the above selections, 4 and 5 are each to be replaced by *c*, and 7 by *e*, so that the three examples cited will become

abc^3e^2k , c^4dfk^2 , ad^2g^5 , respectively.

Each of the selections will then give rise to one product of the r th degree in the n letters; no two selections can give the same product, and no product will be omitted (for it is easy to see that if a product be written down at random, it gives rise to a unique selection, e.g. $b^2d^3fg^2$ becomes $b2d45fg8$).

Hence

$$a^H_{-5} = a_{+5-1} C_{51}$$

N. B. C. DOCKERAY

REVIEWS.

Science and First Principles. By F. S. C. NORTHRUP. Pp. xiv + 299. 12s. 6d. net. (Cambridge University Press)

The author, who is Assistant Professor of Philosophy in Yale University, displays one highly important qualification for the task he has undertaken. He has read very widely in the literature of science, and is able to quote effectively both from the original papers and the expository writings of the chief authorities. The book is seldom dull, except when the "macroscopic atom" creeps up—of which more anon. Perhaps the happiest quotation is one from Poincaré, referring to the application of the laws of probability in physics :

" You ask me to predict the phenomenon that will be produced. If I had the misfortune to know the laws of the phenomena, I would not succeed except by inextricable calculations, and I should have to give up the attempt to answer you ; but since I am fortunate enough to be ignorant of them, I will give you an answer at once. And what is more extraordinary still, my answer will be right."

Much may be forgiven to the man who saves this passage from oblivion.

I will not touch on the later chapters dealing with Life and Man. The physical portion of the book is mainly occupied with relativity ; and although Professor Northrop is very much influenced by the views of Whitehead, unlike Whitehead he accepts Einstein's general theory of relativity. Quantum theory is only briefly dealt with ; but what he says is mainly sound and to the point. He is not always kind to the present reviewer ; in fact, on p. 29, I find myself referred to as Judas.

His main deviation from orthodoxy is due to his not having grasped a rather difficult but indubitable result of Einstein's theory. He is impressed by Whitehead's argument that for measurement to mean anything at all a basis of metrical uniformity is required :

" I cannot understand what meaning can be assigned to the distance of the Sun from Sirius if the very nature of space depends upon casual intervening objects that we know nothing about."

Whitehead considered this a sufficient reason for rejecting Einstein's space of variable character ; and Northrop, though he does not reject Einstein's space, considers it necessary to supplement it with some extraneous reference to supply the basis of uniformity. Both have overlooked the fact that a basis of metrical uniformity is contained in Einstein's theory as it stands, being secured by Einstein's law of gravitation. The law definitely asserts that unoccupied space is homogeneous and isotropic as regards a certain metrical characteristic however heterogeneous it may be in other respects ; and this uniformity is undisturbed by the "casual intervening objects" of which Whitehead speaks. We need not take seriously Northrop's speculations on a macroscopic atom, which in some indefinite way is supposed to give a basis of uniformity to our measurements, seeing that he has not examined the potentialities of the metrical uniformity already contained in the theory. Unfortunately a great deal of his book is coloured by this speculation, and there are continual references to microscopic variability coupled with macroscopic uniformity.

The author discusses the later developments of relativity, the spherical universes of Einstein and de Sitter, and the unified field theories of Weyl, Eddington and Einstein. It appears to me that this discussion confuses philosophy and physics. If I may take his comments on my own unified theory for example, it is difficult to tell whether he is arguing against the physical theory or the philosophical view of the world which I suggest as an outcome. According to him, I "add on so many hypotheses" ; but so far as the physical theory is concerned, it is derived (like the other unitary theories)

by dropping hypotheses, not by adding them. On examination, "so many hypotheses" turns out to be *two*. One is that the mathematical system (introduced in my theory) is the real world (the phraseology appears to be Northrop's not mine). In other words, I suppose there is some kind of truth in my theory—not an unusual hypothesis for a scientist to make. The other hypothesis is that there are consciousnesses "which select out of the possibilities which the most generalised mathematics permits, the particular restricted part which is the metric of our actual world". Every mathematical physicist must make an implicit or explicit hypothesis as to the relation of the formal mathematical scheme to the experience of a conscious being; it appears to me advantageous to try to indicate the relation explicitly. Obviously these "hypotheses" or definitions of the relation of the mathematical scheme to truth and experience ought to be distinguished from hypotheses internal to the various unitary theories which are being compared. It is difficult to credit the author with any understanding of the theory itself, and there seems to be no excuse for the absurd misstatement: "Later he [Eddington] defines a displacement as the 'comparability of proximate relations'".

The reader of this type of book does not expect to be able to agree with the author throughout, and is not likely to trust him as an infallible guide. He will value flashes of insight more than uniform dependability. If read from this point of view the book contains much of value. A. S. EDDINGTON.

The Logic of Discovery. By R. D. CARMICHAEL. Pp. v + 280. (No price.) 1930. (Open Court Publishing Company, London)

This book attempts to deal with the foundations of scientific knowledge, from the standpoint of discovery. It is, of course, plain that the processes of discovery and the subsequent structural development of scientific ideas are totally different domains for study. How far the first of these is amenable to logical treatment is problematical. This book represents an attempt and as such has some value: possibly the degree of success attained is but a measure of the inherent difficulty of the subject. D. M. W.

The Pastures of Wonder. By C. J. KEYSER. Pp. xii, 208. 14s. 1929. (Columbia Univ. Press; Milford)

In spite of the title, this book deals with two serious questions, the nature of mathematics and the nature of science. There is not now any dispute on the first of these, and the author is attempting an easy exposition of accepted doctrine: mathematics is the study of hypothetical propositions. The second question is complicated by the immense variety of contexts in which the word "science" is used, as well as by the diversity of opinions to which great scientists have given expression. The author's proposal is that science is the study of categorical propositions. An attractive balance and completeness is secured if this definition is acceptable, and the case is argued skilfully, sometimes by means of a dialogue with the reader or with an imagined disputant. Only an extract can give an idea of the style. "Without propositions the dance of human life could not go on. Propositions are present in our play and in our work, when we are awake and when we dream. Many millions of them are uttered daily by the men, women and children of the world; think of the untold billions that have been uttered in course of all the ages since human speech began and of the untold billions that will be uttered in the course of future ages before human speech shall end." The colloquialism is deliberate, and we must repeat that the work is serious and deserves attention. The price is outrageous. E. H. N.

The Teaching of Elementary Mathematics. By C. GODFREY and A. W. SIDDONS. Pp. xii, 332. 6s. 6d. 1931. (Cambridge University Press)

This book deals with the teaching of mathematics from the time a boy

enters his Preparatory School till he is ready to take the School Certificate. The treatment falls into three sections. The first, which occupies about fifty pages, is an essay by the late Professor Godfrey on the place of mathematics in education and describes the aims and ideals which have inspired the modern movement which has revolutionised elementary work. It is stated that it was written in 1911, but it is just as relevant to present-day problems as if written yesterday : a striking testimony to the long views and the balanced judgment which characterised all Professor Godfrey's work. Those familiar with the *Reports* of the Association which Professor Godfrey helped to draft, will find here the source of many of the general considerations which have shaped their character and made their influence so salutary.

Naturally there is much in these pages which would be out of place in a formal report ; there is a lightness of touch and even a flippancy of illustration which make it all very good reading, and there is a note of individuality and occasional speculation which supply the type of stimulus committee-made documents inevitably lack.

Apart from a short appendix on more advanced work which seems a little out of place and out of scale, the rest of the book is by Mr. Siddons, though parts of it were written before Professor Godfrey's death in 1924 and were criticised by him.

The second section of the book consists of about twenty-five pages on "General Teaching Points" such as the strategy and tactics in lesson-making, the problem of marks and correction, "rough" work, etc. These notes will be of the greatest value to those who have not yet had much practical class-room experience, and the detailed account of what Mr. Siddons calls the method of "The Nine Questions" will interest every teacher to whom it is new.

The third section, which occupies more than two-thirds of the book, is concerned with the detailed methods and syllabuses of arithmetic, algebra and geometry, taken successively in this order. These chapters, apart from some preliminary discussions, are probably not intended to be read at a sitting ; they form a work of reference to which teachers may turn with advantage when wishing to find an account of the treatment of a special theme, for example, the introduction of decimals, the principles of graphical work, the area-theorems of geometry, and so on. Where there is any considerable difference of opinion among experienced teachers about procedure, this fact is duly noted and the contending arguments are reviewed. Thus Mr. Siddons holds that the approach to algebra should be made *via* the equation and problem, rather than *via* the formula, and he sets out the grounds on which this judgment is based, but he calls attention to the contrary view and points out that neither method can be pursued to the exclusion of the other. The step from the use of a formula to the equation is of course a short one, and the decision each teacher makes will no doubt depend on which method seems to him to give the greater variety of illustration and to link up the work of the class-room most strongly and vividly with the pupil's everyday thoughts.

It is the discussion on the teaching of geometry that the present reviewer has found most interesting and sometimes provocative, which is no doubt what Mr. Siddons intended it to be. For example, his account of the first term's work in geometry ranges over a course that many teachers will consider requires nearly a year, and his syllabus for the first three terms probably occupies two years in many schools. He prefaces his account of the second year's work with the warning that boys must not be rushed on to this before they are sound in the previous work, but he does not indicate how much time he thinks is advisable for an adequate training in the use of instruments, which some teachers consider to be the dominating factor in the problem.

Space does not permit of reference to the numerous practical points in teaching these pages contain ; but here is a sample that will probably encourage

teachers to examine the book for themselves. Mr. Siddons is describing a lesson on the construction of a common tangent to two circles. He says :

" I always draw two circles, centres A and B say, and draw by eye a common tangent ST . ' Now imagine that the two circles are two rotating discs and that ST is a planing machine that cuts both discs away at the same rate.' Draw out from the class that ST will move parallel to itself. ' What will happen ultimately ? ' This leads pleasantly to constructing the circle with radius equal to the difference between the radii of the given circles ; also it leads up to the idea of parallel translation which will be of use later. For the interior common tangent, we have to imagine a wonderful machine that planes at one end and adds on stuff at the other, but the class enjoys that."

This is unquestionably a book every teacher of elementary mathematics should read and keep for reference.

C. V. D.

The Teaching of Elementary Algebra. By C. V. DURELL. Pp. viii, 136. 3s. 6d. 1931. (Bell)

No one concerned with the teaching of elementary mathematics has any business to leave this book unread—or undigested. The young teacher, for whom it is primarily meant, will find it a safe guide, and the experienced teacher, even if he differs, will find useful suggestions and stimulus to thought and observation.

As regards any school subject, there are two things to be considered : the general spirit in which it is to be treated—especially the spirit in which it is to be begun ; and the details, small perhaps but cumulatively of great importance. As to the former, the last generation has gradually brought about a radical change, which Mr. Durell attempts with much success to define, and which his book should greatly help to complete.

His actual definition taken in isolation may sound cryptic. " Broadly speaking ", he says, " the initial purpose is two-fold : discovery and expression ", but as he fills out the meaning it becomes luminous. And as regards the initial difficulties of notation, which are in fact much more than mere difficulties of notation, he says : " The work will fail in its purpose if it does not satisfy the following conditions :

- (i) It must give the pupil constant practice in clear thinking ;
- (ii) It must train . . . his powers of expression and improve his writing of English . . .
- (iii) There must be frequent opportunities for making easy and simple checks . . .

(iv) There must be continual reference to the affairs of everyday life. . . . "

Probably all good teachers will recognise the truth and relevance of this and be glad to have it clearly stated ; the difficulty is to get it applied in detail. And here Mr. Durell is exceedingly helpful. He lays great stress on starting from the " Formula ", but many of us, even if we accept the general principle, may find it hard to get examples sufficiently varied, and above all sufficiently easy. Mr. Durell makes the way plain, though he rightly adds : " teachers should be able to improve on any book they use by adding to it their own ideas and forming their own collection of special illustrations " ; which perhaps we may paraphrase into : there are pupils who seem to prefer a lift to a ladder ; teachers must not set them the example.

The book covers the ground " Algebra up to Quadratics ", and throughout—as to problems, equations, factors, fractions, graphs—the reader will find help. In conclusion, after emphasising the necessity for revision, Mr. Durell recurs to his fundamental point : " This revision . . . will not create the atmosphere which should have surrounded the initial oral development of the subject. It is the view which is first presented to the pupil that matters most, and makes the deepest impression. Interest must be aroused and sustained

from the start, and this is done if, and only if, the part Algebra plays in every-day life is made the dominant theme".

There are two matters on which we are disposed to differ from Mr. Durell: on the one slightly, on the other more seriously.

The former is as to starting with the "Formula": here we do not so much differ from him as think that perhaps he overstates the difference between starting with the "Formula" and starting with the "Problem": of course, if, as in one place he puts it, the conflict is between Formula and Equation, that is another question.

The latter is as to the introduction of negative numbers: here Mr. Durell will tolerate nothing but "directed numbers": "my own opinion", he says, "is that it is wrong, and inexcusably so, to allow or teach pupils to use symbols to which they attach no meaning"; and again, "There is no justification for teaching a pupil that twice (-3) is (-6) before he has learnt to attach a concrete meaning to the symbols (-3) and (-6) ". One hesitates to differ from Mr. Durell, but we cannot but think that here he has gone beyond the mark. This is not the place to argue the matter but we invite attention to one specific statement: "it is just as meaningless to speak of adding a directed number to a signless number as adding a pint to a shilling". This sounds convincing, but is it true? Surely, for instance, the population of a city is a signless number, but we may quite well start a problem with the statement "Let x be the increase of population"; here x may be positive or negative, and so a directed number, yet $p+x$ seems to have meaning.

W. C. F.

The Teaching of Mathematics in Secondary Schools. Vol. I. Technique. Pp. 239. 9s. 1930. Vol. II. Problems in Teaching Secondary-School Mathematics. Pp. 348. 13s. 6d. 1931. By ERNST R. BRESLICH, Associate Professor of the Teaching of Mathematics, The School of Education, The University of Chicago. (University of Chicago Press)

The first of these two volumes contains much simple material which might be taken for granted. The limit is reached in Ch. V, "The Mathematical Equipment, its Use and Care", in which even paper-fastener and blackboard pointer are illustrated. "Three pointers are sufficient for a classroom." In presenting the material, too, analysis and systematisation are often carried beyond the point of usefulness. In Ch. VIII, in which the aims of teaching secondary school mathematics are discussed, a list of nearly two hundred "objectives" under headings and subheadings are given. On the whole, Vol. I contains little to recommend it to the British reader.

Vol II is a thorough and systematic consideration of the main problems in the teaching of elementary mathematics. It lacks the wide philosophic treatment of, say, Nunn's *Teaching of Algebra*, and does not draw upon the history of mathematics as a factor in teaching. But the methods of teaching which it advocates are based on modern ideas, and there is a real classroom flavour about them; the average class teacher would find it a useful guide, but some of the matter would be tedious to the experienced master and to the thoughtful student in training. There is a comprehensive bibliography for each chapter, but it is practically entirely American. The book is suitably indexed.

Chapter III deals with the teaching of arithmetic, and stresses the need for making pupils see the real nature of the processes they carry out, processes which are often obscured by the use of technical terms, such as, e.g. "canceling". Chapter IV discusses the teaching of "fundamental concepts" based on the principle that their meaning should grow out of the experience of the pupil. One or two cases such as "angle", "negative numbers", "area" are considered in detail. In Chap. V, "How to begin the study of algebra", the discussion of the different methods is hardly adequate, but the suggestion of

combining these methods on a geometrical basis is sound. There is a very lucid account of the treatment of directed numbers in Chap. VI which deals with the processes of algebra, while Chap. VII, "Teaching how to solve an equation", is also very clear, and treats the question of "transposing terms" fully. Chap. VIII deals with "verbal problems". Chap. IX, on Functions, Formulas and Graphs, emphasises the need for "functional thinking" throughout the course, beginning with arithmetic.

The next three chapters are concerned with the teaching of geometry, and the treatment is based on the three stages approved by the Mathematical Association. In "Intuitive Geometry" "emphasis is to be placed on the method of obtaining knowledge by seeing, observing, recognising and inspecting", but "it goes beyond that and carries on from study in a systematic way, laying the foundation for the later study of demonstrative, or logical, geometry". These two aspects are worked out in some detail in Chap. X; for instance, classroom treatment of congruence and similarity is suggested in two separate steps, viz. development and application of principles. Chap. XI deals with "Demonstrative Geometry", discussing procedure in teaching the form of a proof and how to study it, methods of proof (analytic, algebraic, indirect), and the treatment of certain topics, *e.g.* loci, symmetry, ratio and proportion. A good chapter on solid geometry follows. The teaching of trigonometry and logarithms is considered in Chap. XIII. In each case the proposed treatment is based initially on the practical experience of the pupil. R. S. W.

A Manual of the Slide Rule. By J. E. THOMPSON. Second printing. Pp. vii, 220. 5s. 1931. (Chapman & Hall)

The Macmillan Table Slide Rule. By J. P. BALLANTINE. 2s. 3d. 1931. (Macmillan)

(1) The author calls his work a "manual"; "textbook" might perhaps be a more fitting term. The book is interesting from the earliest paragraphs. In the opening pages we are given, what we rarely see in books dealing with the slide rule, an account of its history and development, which he traces from the invention of logarithms by Napier in 1614 to the invention in 1850 by Lieut. (afterwards Colonel) Amedée Mannheim of the rule which is the basis of the modern slide rule.

The theory and the method of using the modern instruments are very clearly explained. The modified forms, as the Polyphase, the Chemist's, the Surveyor's, and forms of cylindrical slide rules, all find mention in the descriptions.

Here one feels one must offer a criticism. Since it is necessary to refer frequently to the plates or pictures of the various slide rules, unless it happens that one possesses a model of each form, it would be a distinct advantage if these plates were reproduced upon a larger scale, even if it meant folding them into the book.

The author has not omitted to discuss the working out of typical problems; there is a very large assortment of them, but they are mostly in general or algebraical language. In going through them one felt that each would have been increased in value had it been accompanied by a concrete example and the answer.

The book is written for American readers and "the types of slide rules described are those mostly used in the States", but what is said can be applied easily to any other make of slide rule, and anyone who is interested in slide rules will find much of value in this "manual".

(2) When mention is made of slide rules one pictures at first the compact ruler arrangement which slips easily into the pocket. In the Macmillan Table Slide Rule there are four plates ($11'' \times 8\frac{1}{2}''$), one each for multiplication, powers, sines and tangents, and four slides, being two for multiplication, one for

division, and one for square root. On each plate there is a table of figures in 20 columns of 100 rows, while on each slide there are 10 columns of 100 rows. These tables are based upon the anti-logs to the base 10, those on the plates are respectively anti-log x , anti-(log log x), anti-(log sin x) and anti-(log tan x), while those on the slides are anti-log x , the order being reversed on the division slide, and the values doubled upon the square-root one.

With these, cards are issued "Directions for Use" and "Elementary Instructions". "Elementary" is a comparative term. The tables are most ingenious compilations and the use of them most fascinating. Once one masters the directions and instructions, all manner of computations can be done. Results are obtained directly to 3 and 4 figures, and by interpolation a closer approximation can be obtained.

E. J. A.

Simplified Geometry. By C. V. DURELL and C. O. TUCKEY. Pp. x, 280. 4s. 1931. Or in three parts, 1s. 6d. each. (G. Bell & Sons)

Shorter Geometry. By C. V. DURELL. Pp. 156. 3s. 1931. (G. Bell & Sons)

"A First Geometry" would, perhaps, be a better description of the former of these two books. For there is no attempt to "simplify" in the sense of to evade difficulties proper to the stage for which the book is intended. Its three parts each provide roughly a year's work, and it starts from the very beginning. It is thus suitable for preparatory and central schools and for the lower classes of secondary schools. The first part is an exploration of the contents and possibilities of a box of geometrical instruments. The instruments are studied one by one (ruler, compasses, set-squares, protractor), and most careful directions are given for the use of each. This is all very valuable. It is surprising, in these days when children are taught to use their hands in so many different ways, how many boys arrive at Public School age with the clumsiest habits of handling geometrical instruments. We are painfully familiar with the blobs to represent points, the holes in the paper made with the butt-end of a pair of compasses, and the imperfectly straight lines drawn with unpointed pencils. This part of the work could not be better done than it is in this book. Consider, for instance, the wisdom of this paragraph: "A rough figure does not mean an untidy figure. It may be drawn entirely freehand if it can be done neatly; but otherwise straight lines should be ruled. The figures should be large, much larger than the figures in this book". Or this: "Accuracy in drawing and measurement depends on taking trouble and using proper methods".

This part is not restricted to mere drawing and measurement. General principles are induced empirically. Thus several pairs of parallel lines are drawn with set-squares, and several sets of alternate angles are measured. Hence it is rendered "very probable" that alternate angles are always equal. The exercises contain many one-step deductions. Solid figures are freely used as illustrations and in exercises, and the use of loci is introduced through the idea of a search for buried treasure.

In Part II a systematic development of geometrical properties on informal lines is begun. Thus, starting with the assumed facts about parallels and angles, the fact about the angle-sum of a triangle is reached. This is done carefully; first of all questions for class-discussion, then numerical exercises leading up to the general fact by a series of deductive steps. We thus arrive at what has been called the "bunch of bananas" stage of Geometry, in which a cluster of theorems hang from a single stalk. There is the same care and practical wisdom shown in this part as in Part I. Such things as the importance of order in naming the points describing congruent and similar triangles, the correct style in setting out a proof, the importance of using recognised abbreviations and of indicating reasons at every deductive step, are stressed. The advice is given in such a straightforward and sensible way that it should impress the most careless reader. The "examples for class discussion" are

really a set of excellent class lessons, each on a particular point. No teacher who follows these lines can go wrong. Part II thus deals with angles of plane figures : symmetry, congruence, and similarity ; areas ; angle properties of a circle ; the theorem of Pythagoras ; similar figures and solids. This reaches the standard of the "Common Entrance" examination. Part III continues on the same lines, and extends the work to cover applications of congruency, intercepts, ratios, and tangent properties. Rather more stress is laid in this part on the presentation of formal proofs. The complete three-part course corresponds to the syllabus for entrance scholarships at Public Schools. This book is most happy in design and admirable in execution. As one would expect from the collaboration of two such skilled teachers, it is based on wide experience, it is well-balanced, and the exercises are full of interesting things, and are well graded. The following criticisms on minor points are offered without any wish to detract from the general excellence of the book.

A section might well have been devoted to the drawing of solid figures, that is, to the best methods of representing pyramids, prisms, cuboids, intersecting planes by perspective figures. Many pupils find difficulties here. Teachers know only too well the dilapidated haystacks which are meant to represent pyramids on a square base, and cuboids which look unconvincing because all the lines are drawn equally thick.

In Part I formal definitions are rightly eschewed, and common-sense words used, when possible, in place of technical terms. But this practice is not without its ambiguities. Thus on page 13, a quadrilateral is said to be a figure with four sides. On page 15, example 5, a quadrilateral is shown with its angular points joined to a point in the middle of the figure. The pupil is expected to say that the number of sides is now 8. In the same exercise "corner" is used as a point of intersection of two or more straight lines, whereas on page 26 "corner" is identified with "angle".

Some of the text would have been clarified had the distinction, advocated by Mr. Forder, between "interval" and "line" been made from the start. The distinction made on page 66 between "true" and "compass" bearings is not quite accurate, as it leaves the impression that N. 20° W. and 340° indicate the same direction. A footnote should be added to explain that "true" bearings are measured from 0 to 360° clockwise from the True or Geographical North, and that Compass bearings, given from 0 to 90° East or West from the North or South points, are with reference to the Magnetic North. The two North directions are not normally the same, owing to magnetic variation. Incidentally, pupils in South Africa and Australia will go wrong if they follow the directions on page 47 for finding the North point.

A warning might have been given as to the approximate nature of all answers based on drawing and measurement. One knows the confidence with which a boy will measure a line to the nearest tenth of an inch, and then give up the answer for one-seventh of the length to six decimal places. It is particularly important to be careful when the area of a rectangle is being obtained from the product of two measured sides. If the sides are measured to the nearest hundredth, the result is not accurate to four decimal places. It is better to say that an area lies between 6.3 and 6.4 sq. in. than to say that it is 6.3456, the last two figures being meaningless. The numbers given in the newspapers to represent the speeds obtained in the Schneider Cup and similar speed-tests are bad examples of the futility of this sort of thing.

If Part III is intended for the use of pupils preparing for scholarships at Public Schools, it would be a help if there were added a set of harder miscellaneous deductive exercises, such as are often set at these examinations.

Shorter Geometry continues the course up to the standard of the School Certificate examination. The treatment is now more formalised—the "bunch of bananas" stage has given place to that of the "string of sausages". The text of the theorems and many of the exercises are taken from the author's

well-known *Elementary Geometry* (some of the material of which has also been incorporated in the *Simplified Geometry*), but the harder exercises have been omitted and many new easier ones inserted. The idea of a set of numerical exercises on a group of theorems, followed by a set of deductive exercises, is a good one. The merits of *Shorter Geometry* are those of the earlier book. The proofs are well set out and the general arrangement is admirable. It is the sort of book which is very easy to handle for class purposes. There is one important change in order from the earlier to the new book. The fundamental theorems on congruence and parallels now occur near the beginning of the sequence, whereas in *Elementary Geometry* they were consigned to an Appendix. The proofs by superposition and Playfair's axiom are given without any note as to logical difficulties involved. These theorems are, however, starred for omission by pupils who are being prepared for examinations in which the proofs are not demanded. Though the book contains all that the average school-pupil will probably require for the purpose of the School Certificate or a similar examination, pupils of more than average ability will certainly need harder exercises (such as those contained in the larger book), and should have more careful and leisurely discussion of underlying logical principles and difficulties. In these Relativity days, no school Geometry which may be used by bright young people with inquiring minds should be without a paragraph (in connection with parallelism) on non-Euclidean Geometries and their implications.

Both the books are very well printed, but neither contains an index.

H. E. P.

Projektive Geometrie. By L. BIEBERBACH. Pp. vi+190. Rm. 7.80. 1931. Teubners Mathematische Leitfäden, 30. (Teubner)

This booklet is a sequel to the author's *Analytische Geometrie* in the same series (see *Gazette*, vol. xv, p. 271). The opening sections show that the deliberate homogenisation of coordinates produces apparatus by which some problems are handled more readily than by cartesian methods; among examples are isogonal conjugates in a triangle. The homogeneous coordinates are seen to imply a system with an axiomatic basis of extreme simplicity: *Any* two lines in a plane intersect in one and only one point, and so on. This is said to be the basis of all that follows, but inevitably, considering the scale of the treatment, the converse process of introducing coordinates on the axiomatic basis alone is taken for granted, and in spite of a reference for the formal development to "die beste heute existierende Darstellung: Veblen und Young, *Projektive (!) Geometry*", and a remarkably clear paragraph on the Erlanger Programm, the work is certain, I think, to produce on anyone coming to the subject for the first time the impression of an essay on the use of homogeneous coordinates in the study of conics and conicoids. There are abundant examples of the theorems which result from the introduction of the line at infinity and the circular points, and the distinction between real and imaginary is kept clear*. The booklet is to be recommended warmly, and readers will be glad that a third volume is promised.

E. H. N.

Projektive und nichteuklidische Geometrie. Bd. I. Projektive Geometrie in analytischer Behandlung nebst einem Einblick in die Grundlagen der Geometrie. Pp. xi, 209. Bd. II. Nichteuklidische Geometrie auf der Grundlage der projektiven Geometrie. Pp. xi, 214. By F. SCHILLING. Rm. 13.60 each. 1931. (Teubner)

This is not a treatise on either projective geometry or non-euclidean geometry, but a most careful development of the principles of non-euclidean geometry from the axioms and fundamental concepts of projective geometry.

* It is surprising to find a directrix defined as the polar of a *real* focus. There is no difficulty in crediting an imaginary focus with an equally imaginary directrix.

Friedrich Schilling wrote up Klein's lectures on non-euclidean geometry in 1889-90 while he was his assistant at Göttingen. The present work, which is based on lectures given at the Technical Hochschule in Danzig in 1923-4 and 1927-8, is suffused with the spirit of Klein's geometry and is a true work of art. To Klein we are indebted for the working out of two great ideas: the reduction of metrical geometry to projective, and the description of the various geometries in terms of the continuous groups which they admit. Projective geometry, independent of any parallel-axiom and purified of all metrical ideas, is the simplest organic structure, out of which by the grafting of new shoots the more complicated structure of metrical geometry can be gradually moulded.

The first volume deals with projective geometry, and is based on Hilbert's axioms of connection and order. In view of the later developments, however, there is given a preliminary explanation of "improper" elements: points and lines at or beyond infinity. The necessity of considering such elements, and the difficulty of applying the idea of "between" in elliptic geometry, are removed by confining the discussion to a bounded region such as the interior of a triangle, "das unmittelbar vor unsren Augen liegt". Three straight lines divide the plane into four projectively equivalent regions, distinguished by Pasch's axiom; it is immaterial which of these is considered as the interior of the triangle. Desargues' Theorem (on perspective triangles), which follows from the axioms of connection, once three dimensions are admitted, is applied with great effect in the construction of a line through an inaccessible point, and also in proving the uniqueness of the harmonic quadrilateral construction. The projective system of coordinates is established for rational coordinates by means of the Möbius net, and extended by the application of "dyadic-rational" numbers, i.e. those fractions whose denominators are a power of 2; to secure continuity Dedekind's axiom is assumed. "The establishment of the coordinate-system in euclidean analytical geometry can be compared to the making of a cabinet with the help of plane, saw, carpenter's bench and all the other tools; in the establishment of the projective coordinate-system on the other hand we have made a very beautiful cabinet in which we can now arrange and exhibit many things in a convenient manner, and all with the help of just a pocket-knife" (p. 92).

After establishing next the equation of a straight line with the help of the "harmonic centre" of a segment— c is the harmonic centre of the segment (ab) when $(abc\infty)$ is harmonic—he remarks (p. 103) that "we have now very calm water to navigate, all the rapids in the neighbourhood of the sources have been safely passed", and the rest of the first volume is devoted to transformation of coordinates, projectivities or collineations, and the projective theory of the conic.

The last chapter deals with some representations of the projective plane on a bounded region.

The second volume is devoted first to the foundations of metrical plane geometry (euclidean and non-euclidean) on the basis of projective geometry. He introduces the idea of *motion* in three axioms: 1 (the projective axiom), Every motion transforms points and lines uniquely into points and lines; 2 (axiom of uniqueness), There is a unique motion which transforms a given line g and a given point S on it into a given line g' and a given point S' on it, and a given sense on g into a given sense on g' , and points on a given side of g into points on a given side of g' ; 3 (group axiom), The resultant of two given motions is another unique motion. By the special motion of reflexion in a line he arrives at the idea of the absolute pole G of a line g , and defines a line $h \perp g$ when h is transformed into itself by reflexion in g . (This definition is too wide, since g also is transformed into itself. The definition should read: $h \perp g$ when h passes through the absolute pole of g .) A whole page is required to prove that then also $g \perp h$. After explaining further the motion of reflexion

in a point, he defines angle as a class of ray-directions, and proves the congruence-theorems for a triangle and the equality of all right angles. Thus by the axiom of uniqueness Euclid's method of superposition is given a logical basis. The work here is a little sketchy, as he uses the terms greater and less and the process of addition of angles without definition. A definite parallel-axiom is now introduced. By repeatedly laying off the unit-segment along a line, OA, AB, BC, \dots , either there is a point on the line which does not belong to any of these segments, or this is not the case. In the former case the axiom of continuity demands the existence of a limit-point U (in the projective sense); and by laying off the segments in the opposite sense another limit-point V is obtained. Either U and V are distinct (hyperbolic geometry), coincide (euclidean), or do not exist (elliptic). The absolute polar system is then established, with the circular points in euclidean geometry and the absolute conic in non-euclidean. The equation of the absolute conic ($x^2 + y^2 - \kappa z^2 = 0$ in hyperbolic geometry) is determined, involving the first "space-constant" κ , which is such that the absolute polar of the unit-point is $x + y - \kappa z = 0$. The expressions for the measure of an angle and a segment are then determined in terms of cross-ratios. For the angle, taking the measure of a "straight angle" as π , $\phi = \frac{1}{2}i \log(uvab)$; for a segment $AB = \frac{1}{2}\mu \log(UVAB)$. The second space-constant, μ , is connected with κ , in hyperbolic geometry, by the relation $\tanh \mu^{-1} = \kappa^{-\frac{1}{2}}$.

Part II of the second volume deals with the circle, the element of arc, non-euclidean trigonometry, and the area of the triangle. The trigonometrical formulae are obtained by direct analytical calculation. The area of a triangle is based on the theorem that triangles of equal angular excess (or defect) are "equal by dissection" (zerlegungsgleich). In elliptic geometry the basis is the finite area, $2\pi\mu^2$, of the whole plane. In hyperbolic geometry the discussion is based on the area of a "triply asymptotic" triangle, which is proved to be constant and finite and equal to $\pi\mu^2$.

The work concludes with remarks on the consistency of the axioms and the bearing of relativity, geodesy and astronomy in deciding the probable value of the space-constant.

The two volumes are beautifully printed, the diagrams being particularly fine. A few misprints have been noted: Bd. 1, p. 120, ρ_2 for ρ_3 ; Bd. 2, p. 66, a wrong sign in equations 16a and 17a; p. 96, equation 13c, s_2 for s_1 ; p. 117, equation 16', x_m for κ_m . D. M. Y. S.

A School Algebra. By A. M. BOZMAN. Pp. vi + 427 + lxii. 4s. 6d. 1931. (Dent)

Of the first part of this book it may almost suffice to say that it takes rank along with other books, recently reviewed in these pages, as a sound introduction to Algebra. The first chapters are devoted to the Formula and to the use of Algebra as generalised Arithmetic.

The treatment of graphs and negative numbers follows the lines now generally accepted.

This part includes the Remainder Theorem, long square root and literal equations, but has no chapter, except a very elementary one, on Fractions.

The second part covers the syllabus of the additional mathematics of the School Certificate, and includes a chapter introducing the ideas and methods of the differential calculus by means of the gradient of a graph.

In this part the author is not on such firm ground. On p. 253 appears "The square root of a rational number and a surd can be written as the sum of two surds, i.e. $\sqrt{x + \sqrt{y}} = \pm(\sqrt{a} + \sqrt{b})$ ". The wording is loose and the statement only true if $x^2 - y$ is a square number. On p. 317, the function $xy(x - y) + yz(y - z) + zx(z - x)$ is said to be symmetrical in x, y, z when it does not satisfy the definition of symmetry given on p. 316. On p. 344, in

the statement "The graph of $y=kx^{-n}$ is a discontinuous curve, with the axes as asymptotes, for all negative integral values of $-n$," some amplification of the word "discontinuous" seems desirable. On p. 366, $(\alpha+i\beta)(\alpha-i\beta)$ is made equal to $\alpha^2-\beta^2$, and the slip is the author's, not the printer's.

It would not be difficult to correct these lapses and also a misprint in the headings of pp. 342 and 343; but pages 407-8, devoted to a proof of the Binomial Theorem for negative and fractional indices, require drastic treatment, preferably excision. There is another lapse fundamentally serious. On p. 278 the statement is made that "the property of proportional parts enables us to find the value of five-digit numbers from the ordinary four-figure tables" and is followed by two worked examples, producing .89302 as the log of 7.8162 and 1.31058 as the log of 20.446.

At this stage the pupil who does not realise that results obtained from four-figure data are not reliable to four figures, much less to five, will find in his work with logs plenty of illustrative examples.

Thus: $\log 2 = .3010$ and $\log 3 = .4771$; whence $\log 2 \times 3 = .7781$, which is antilog 5.999.

In the examples mentioned above, the data used contain in themselves evidence of this unreliability. Thus if $\log 7.817$ had been obtained by subtraction of a difference from the $\log 7.82$, it would have appeared as .8930 instead of .8931, and the example would have peters out.

A comparison with seven-figure tables would have been instructive. The following example has been chosen as an extreme case:

$$\begin{aligned} \text{From the four-figure tables} \quad \log 4.290 &= .6325, \\ &\log 4.291 = .6326, \end{aligned}$$

whence by the principle of proportional parts $\log 4.2904$ would appear to be .63254. Now seven-figure tables give $\log 4.2904$ as .6324978, which is less than .6325, used as $\log 4.290$.

We have dwelt on this point at some length, because it is important in all work with approximate data that the pupil should realise the limits of accuracy thus imposed on his results.

In spite of these blemishes, the book is extraordinarily attractive. It is inspired by a full understanding of the difficulties encountered in the classroom, and is designed to guard against or overcome them. The explanatory text is compact and to the point, and is so set out that it should be easy to understand, retain and reproduce. Thus pp. 255-6 on indices are patterns of good statement of algebraical propositions. Wise cautions appear, e.g. "Check in the original question. It is not enough to substitute in the original equation. The equation itself may not be correct".

And although a wide syllabus is covered in a comparatively small and certainly low-priced volume, there is no skimping of explanation or of examples. Nor is there any crowding of the matter; indeed, the lay-out has a spacious appearance that is not only pleasing but helpful.

F. C. B.

A Junior Algebra Complete. By DENHAM LARRETT. Pp. 258 + answers, 4s. 6d.; or without answers, 3s. 6d. 1931. (Harrap)

In the preface the author announces that he has tried to provide something for the beginner to read and work through, as far as possible, unaided. Teachers who sympathise with this aim, or are compelled to pursue it, may welcome the result. The type and lay-out are admirable. In Part I the explanations given are elaborate, and the steps are taken at an easy pace. So careful is the author to make the pupil's way easy that he devotes a short chapter to consecutive numbers.

From the beginning he appeals to the pupil's acquaintance with arithmetical processes in order to introduce him to algebraical ones. He pursues this method even for the long square-root method, of which he says: "It is

modelled on the corresponding process in Arithmetic, the only difference being that the square root of the first term at each stage of the process is evaluated exactly. Consider the following example ", and so on.

A quotation from a problem will indicate the author's treatment of simple equations :

$$2n - 500 = 4$$

$2n = 4 + 500$ (moving 500 to the other side of the equation
and adding it instead of subtracting).

The author writes the corresponding rules, and repeats them *in extenso*, so that the pupil may have no excuse for forgetting them.

Graphs are introduced by some talk on axes and the plotting of $y=2x+3$. This leads to the consideration and solution of simultaneous equations.

In the same way the study of the parabola introduces the study of the quadratic, including some in two unknowns that form an excuse for the graph of $x^2 + y^2 = r^2$. But before the pupil reaches this culmination of the work, he has read and worked through a chapter on Rationalising Denominators and on Partial Fractions. He has been spared a chapter on Change of Subject in a Formula.

There is no reason to doubt that an intelligent pupil might acquire the mechanism of elementary algebra by an almost unaided study of this book. But whether it would give him any grasp of the spirit or logic of the subject, the reader of this review may judge for himself.

F. C. B.

Moderne Algebra (zweiter Teil). By B. L. VAN DER WAERDEN. Pp. vii, 216. Rm. 15 (geb. 16.60). 1931. Grundlehren der Mathematischen Wissenschaften in Einzeldarstellungen, xxxiv. (Springer, Berlin)

In volume II the author carries out the task outlined in his volume I (reviewed in *Gazette*, No. 214, p. 437). The general theory having been developed, he proceeds with his more detailed discussion of particular topics, such as elimination, polynomial ideals, linear algebra and hyper-complex numbers.

The whole is competently done, and is, of course, written by an acknowledged expert. Some of the less important theorems and some allied problems appear as examples at the ends of the chapters.

An amusing minor point about the book is a sort of family tree, wherein each branch of algebra is shown with its logical parents somewhere higher up on the tree. It is, in some ways, a pity that in our modern books logic gets all the bouquets and history gets most of the kicks. I should be much more interested in a historical tree than in a logical tree, but I suspect it would take a lot more study to draw one up.

W. L. F.

Stage 'A' Trigonometry. By C. V. DURELL. Pp. 96. 1s. 6d. (without Four-figure Tables). With 24-page supplement of Four-figure Tables, 1s. 9d. Answers, 6d. net. 1931. (Bell)

This book provides a good short course in Trigonometry suitable for use with junior and middle forms of Secondary Schools. It may be recommended with confidence.

It begins with the definition of the tangent of an angle and proceeds, with the help of numerical examples, to the ideas of sine and cosine. Questions involving three dimensions, solution of triangles, the definition of cosec, sec and cot, identities and circular measure then follow—all treated mainly from the numerical point of view. There are plenty of diagrams, and the examples are clearly and concisely worded. The printing and arrangement are both excellent.

Trigonometry Test Papers. By J. W. M. GUNN. Pp. 92, xii. 1s. 1931. (Rivingtons)

This is an excellent collection of 400 graded examples, with answers. The examples are divided into sets, each set containing eight examples. They are designed to meet the needs of the Higher and Lower Scottish Leaving Certificate, and the Oxford and Cambridge Local Examinations. The questions are clearly worded, and the printing is of the high standard we expect from the Edinburgh University Press. We note that the author does not seem to have decided on what his x shall denote. On p. 76 he uses " $\cos(x - 30^\circ) = \sin x$ ", but on p. 77 he introduces parenthesis and writes, "Prove that the number of radians in an angle of 'x' degrees is $\pi x/180$ ".

Plane Trigonometry. By B. B. BAGI. Pp. vi, 248. Rs. 8, 8 as. 1931. (Tutorial Press, Bombay)

This book has been written with the object of meeting the needs of students of Indian Colleges and, since the author is in close contact with these needs, it should achieve this end. It covers the ground of such examinations as the Matriculation examination, but we should not care to recommend its use for this purpose. The printing is heavy and the diagrams are poor. The practice of numbering the examples consecutively—the author's last example is No. 561—is not to be recommended. In his work on solution of triangles the author uses seven-figure logarithms, while he deduces the necessary formulae from Newton's formula—although he gives alternative proofs. Many other authors prefer to use Newton's formula as a check formula. In the later parts of the book, alternative proofs are given which make use of the Calculus. But there is nothing original in the book—the author covers the course which he has in mind in an ordinary, straightforward way.

We say this because the author, in his preface, directs attention to the fact that the majority of Trigonometry books on the market have been written for use in English schools, and in connection with this he states that "the treatment adopted in such books is naturally lacking in vigour and comprehensiveness. It can hardly do justice to the maturer intellect and the higher level of mathematical knowledge of the students of Indian Colleges". We may add that there is, in the book itself, no evidence that the author has any warrant for making this assertion.

First Trigonometry for Schools. By H. ABSON. Pp. ix, 288. 3s. 6d. 1931. (University Tutorial Press)

The contents of this book are, in brief—definitions, solution of triangles, logarithms, circular measure, angles of any size, projection, three dimensions, identities and equations, sum and product formulae.

A feature of the book is the large number of examples in which the author strikes the happy mean between the strictly numerical and the strictly analytical type of example. But we are left with the impression that, in writing his bookwork, the author has not been restrained by thoughts of relevance, and that the omission of all the pointless allusions would have secured a substantial improvement in the book.

We observe that the author makes the common mistake (p. 44) of defining the ratio of like magnitudes, A/B , as being synonymous with $(A/U) \div (B/U)$, and thereby involves himself in circular definition. He is, as a result, wrong when he infers that "it is impossible to talk about the ratio of a magnitude to one of another kind".

V. N.

Spherical Astronomy. By W. M. SMART. Pp. xi, 414. 21s. 1931. (Cambridge University Press)

There has been during the past four or five years a veritable flood of literature on astrophysics. This, perhaps, is not altogether surprising, considering

the tremendous advances the subject has made since the beginning of the century, but nevertheless it has been a source of some wonder why one or two of the enthusiastic authors of these books did not turn from the very over-crowded astrophysical market and write a really useful account of the mathematical side of positional astronomy. Existing books have been either too elementary, or out of print, or old-fashioned and completely out of touch with modern methods. There has been a clear demand for a volume of reasonable size which would include not only an up-to-date account of conventional spherical astronomy, but also some description and explanation of branches of the subject which have in one way or another come into special prominence in more recent years. The book required had to be primarily mathematical and not too elementary, it had to recognise the fact that the principal tools of modern observational astronomy are the spectroscope and the photographic plate, and it had to include an adequate treatment of binary stars, owing to their very great importance as the source of the mass and luminosity data required in all theories of the internal constitution of the stars.

Dr. Smart's latest book goes a long way towards meeting these demands and is on that account very welcome. It is based on two courses of lectures given by the author each year at Cambridge to students preparing for Part II of the Mathematical Tripos. The book thus has an examination background, but it is good to find that, apart from the list of examples at the end of each chapter, no great emphasis is laid upon this fact.

The first chapter includes all the spherical trigonometry an astronomer is likely to require. We are then led on to the usual problems of the subject, but apart from a good account of the meridian circle and the welcome clarity of the diagrams showing the celestial sphere, the earlier part of the book calls for little comment. The more interesting chapters are those later ones in which the author breaks new ground and considers the points with which modern observational work is chiefly concerned, for a text-book of this kind, more than any other, can maintain vitality only by keeping in closest contact with the physical facts of the subject. The sections on proper motions, astronomical photography and the determinations of both visual and spectroscopic binary orbits have particular value for another reason too. The material in them has hitherto existed mainly in original papers, which have formed scattered and inconvenient sources of information, or has been embedded amid other less essential matter in more highly specialised articles and books.

Generally speaking, Dr. Smart has done his work excellently, and only a few points of detail can be criticised. There is a trivial lapse in the English at the top of page 215. Attention might be directed to page 223, where we are told that "parallaxes as small as '005" can be measured with fair accuracy", and to the example on page 312, where a parallax is derived with a probable error '003". This degree of precision is not actually attained even in the best modern observations, and the general consensus of opinion among workers in this field is that the smallest probable error reached at present is around '010". Statements on pages 254 and 298 might lead some readers to suppose that proper motions of stars brighter than the ninth magnitude are never determined photographically, which, of course, is not the case.

In the chapter on binary stars it should be mentioned (p. 340) that optical doubles are rare. Later (p. 356) there is a description of how the total mass of a binary star may be found, but the kind of accuracy to be expected in these very important determinations is not stated. Eclipsing variables are dismissed in a paragraph. The analysis of their light curves may be quite unsuitable for a text-book such as this, but at least it should be brought to the reader's notice that they can give the inclination (and therefore the masses when both spectra are visible) in spectroscopic binary orbits, especially as photoelectric measures during the last few years have demonstrated that very many of these binaries show shallow eclipses.

In one or two places there is an appearance of repetition in the mathematical argument, which one felt might possibly have been avoided. On the other hand, this is a book written from a definitely practical standpoint, so that it is permissible and indeed desirable to put observational realities, rather than mathematical elegance, first.

These, however, are criticisms of detail. As a whole the book is a thoroughly good one and may be recommended without reserve to anyone wishing to study the geometrical and positional sides of astronomy. Dr. Smart is to be particularly congratulated on those chapters, already mentioned, in which he brings together information which is not readily to be found in other English text-books. For the rest, the printing is good and typographical errors seem to be entirely absent, the diagrams are generally very clear, there are some useful tables to be found on the last few pages, and finally there is an adequate index.

R. O. R.

Éléments de Trigonométrie Sphérique. By G. PAPELIER. Pp. 162. 20 fr. 1930. (Paris : Librairie Vuibert)

The astronomical student usually requires only a knowledge of how to derive and use the three or four principal formulae of spherical trigonometry. As indicated above, he will find practically all he wants on about a dozen pages of Chapter I of Dr. Smart's book. The remainder consists chiefly of practice in visualising and drawing suitable figures.

Those who require a closer acquaintance with the more elementary parts of the subject may find this book useful. It discusses fully the solution of spherical triangles, and deals with the various problems analogous to the usual theorems of elementary plane trigonometry, e.g. the question of inscribed and circumscribed circles, medians, perpendiculars, etc. There are also formulae and theorems dealing with the spherical excess. There are a number of examples at the end of the book, where a complete table of formulae may also be found.

A few misprints were noticed, apparently due in every case to faulty type.

R. O. R.

Mathematical Astronomy. By C. W. C. BARLOW and G. H. BRYAN. Fourth edition revised by A. C. D. CROMMELIN. Pp. xvii, 445. 9s. 6d. 1930. (Univ. Tutorial Press)

This well-known text-book has been revised for its fourth edition by Dr. Crommelin. References and examples have been modified to correspond with the alteration of the beginning of the astronomical day from noon to midnight and with the changes in the *Nautical Almanac*, which started in 1931. Sections on the Julian day and the Metonic cycle have been added, and the chapters on the planets and on eclipses have been amplified. Details have been attended to, not only in the text, but also in the examples, in order that this new edition may as far as possible stand in line with modern advances in astronomical knowledge.

R. O. R.

A First Course in Arithmetic. By W. G. BORCHARDT. Pp. viii, 202, xxxvi. With answers, 2s. 6d. 1931. (Rivingtons)

A Junior Arithmetic. By R. C. FAWDRY. Pp. vii, 178, xl. Without answers, 2s. With answers, 2s. 6d. 1931. (Bell)

These books are for use in Preparatory Schools and in junior classes of Secondary Schools ; the opening sections in each are, of course, for revision only. Mr. Fawdry's book has sections on Percentages, Profit and Loss, Interest ; Mr. Borchardt omits these subjects, but he has a section on Graphs. Each book contains a very large number of examples, and each is successful in realising Mr. Borchardt's aim of avoiding "long and tedious calculations". In each book, "shop" subtraction is recommended, but Mr. Borchardt seems

to prefer the "decomposition" method ; his book also includes a useful set of tables of weights and measures.

In multiplication the unit figure of the multiplier is placed under the right-hand figure of the multiplicand, and the operation begins with the left-hand figure of the multiplier. When decimals are involved Mr. Fawdry proceeds as with integers, and inserts the decimal point in the result after a "rough check", and he uses a corresponding method for division—though he mentions "Standard Form". Mr. Borchardt gives this method and two others for multiplication. It is interesting to note (from his preface) that after more than twenty-five years' experience of it he finds the "Standard Form" method entirely satisfactory for teaching purposes—indeed, it is his main method for division of decimals.

It is hardly necessary to add that both books are well printed ; Messrs. Bell use a slightly larger type than Messrs. Rivington, and Messrs. Rivington set out their pages so that their significance is more readily apprehended at a first glance.

T. M. A. C.

(1) *An Introduction to Mechanics.* By J. W. CAMPBELL. Pp. xiv, 384. 8s. 6d. 1931. (Harrap)

(2) *Intermediate Mechanics ; Statics and Hydrostatics.* By D. HUMPHREY. Pp. xi, 438. 10s. 6d. 1931. (Longmans, Green & Co.)

(1) Dr. Campbell has made a very definite attempt to break away from the traditional method of presenting Mechanics. He has an obvious zest for his subject as an important branch of Science, and not as a mere examination subject. His aim is to produce an elementary text-book which gives the student an insight into the fundamental concepts of Newtonian Mechanics, rather than the ability to solve a number of tricky problems with the use of half-understood formulae.

The book is suitable for those starting Mechanics after School Certificate, and also particularly for those students, fortunately a decreasing number, who go up to the University without any knowledge of Mechanics. The previous knowledge of the Calculus required is not great, although it is used throughout. Newton's laws of motion are taken as fundamental and Dynamics and Statics developed side by side from them.

While the general scheme of the book is admirable, there are some points which require criticism. For example, in the otherwise carefully written chapter on Newton's laws of motion, no attention is paid to frames of reference. This may be part of the author's policy of telling the student nothing but the truth, although not the whole truth : but there is no reason why he should not be told that in formulating the laws the fixed stars are used as a frame of reference and that in most terrestrial problems the rotation of the earth can be neglected.

One cannot help regretting that a more thoroughgoing use is not made of the vector calculus. It is most useful in later work if the student has become familiar with a vector notation in two-dimensional problems. The definition of moment about a point at the beginning of Chapter VII is misleading, unless attention is paid to the footnote which points out that what is really meant is moment about an *axis*. The last chapter on "The General Motion of a Rigid Body" might well have been omitted. It is beyond the scope of the book, and vector methods would be an improvement.

There are some minor disadvantages to the reader in this country. The tables referred to in the text are those of the author, and "for some parts of the work there are no other tables which will suffice, and for other parts these tables, though not essential, are more convenient in application than any others available". One feels that reforming zeal may go too far in re-introducing "slug" as the gravitational unit of mass, and there are words,

such as "mediumship" and "anti-differentiation", which are not in common use here.

(2) The *Intermediate Mechanics* covers the Higher Certificate syllabus and part of the course for an ordinary degree; the first volume on Dynamics has already been reviewed in the *Gazette*. The value of the *Statics and Hydrostatics* lies chiefly in an excellent selection of worked examples and a large and comprehensive collection of problems drawn from many sources. The book has proved useful and practical in two large secondary schools known to the reviewer. The methods of the calculus are used throughout and the treatment is non-vectorial. There is a full section on graphical methods, which are omitted by Dr. Campbell. As in the *Dynamics*, little attempt is made at a fresher and more accurate exposition of bookwork than in the older textbooks. For example, the law of Reaction is expressed in the usual perfunctory statement which is so often imperfectly understood by beginners. This is followed by remarks on the "transmissibility of force" which are far from clear. An unfortunate omission is that there is no reference to the method of determining positions of equilibrium by differentiating the potential energy function except when the potential energy is entirely gravitational. In spite of these blemishes, the book is a useful one and seems likely to fill a gap in sixth form mathematical literature.

B. S.

Hints and Solutions to Examples in Mechanics. By A. ROBSON and C. J. A. TRIMBLE. Pp. vii, 52. 5s. 1931. (Bell)

Examples in Mechanics, the forerunner of the subject of this review, is a book intended for scholarship work in conjunction with dictated notes. The absence of any bookwork or worked examples increases the need for hints on the best methods, especially in the case of harder examples; consequently the present volume should increase the use of the book of examples.

In *Hints and Solutions* the authors, as stated in the preface, have not attempted to give a complete key to all the examples, but have supplied the necessary suggestions to reduce the harder examples to the standard of the easier ones. The form of the suggestion is such that it could be given complete to those who had already attempted and failed at any one example, or initially to the weaker boys who have little experience.

Special mention should be made of the sections on Simple Harmonic Motion, Virtual Work and Rigid Dynamics, for while the whole book should be of great value to teachers using the examples, the hints on these sections are particularly good.

E. H. F.

Theoretical Physics. I. Mechanics and Heat. By W. WILSON. Pp. x, 332. 21s. 1931. (Methuen)

The aim of this work, as the author states in his preface, is to present a comprehensive account of theoretical physics, without being too elaborate and voluminous, and he has produced in this first volume a book which is readable and essentially sound. The mathematical knowledge demanded from the reader is modest; and it is supplemented by an introductory chapter on mathematical topics of immediate physical application, such as Stokes' theorem, Fourier series, and a short account of vectors and tensors. This part is necessarily sketchy, and would naturally be omitted by the mathematical reader. It may, however, prove useful to some readers.

The book is written from the standpoint of the physicist rather than the mathematician, and the chief interest lies therefore in the principles and results, rather than in the technique, which is simplified as much as possible. There are three chapters on Newtonian dynamics, and the ground covered includes gyroscopic motion and the theories of Lagrange, Hamilton and Jacobi. The virtue of this seems to be that it presents a collected account of the main topics of dynamics. The treatment of the fundamentals is rather

thin, and as a text-book its value is probably slight in this section. It should, however, prove of interest to those with an elementary knowledge of dynamics, as an introduction to the higher branches.

One of the main charms of the book is that the different topics are treated in such an order and manner as to give the reader a vivid sense of their essential unity. Thus the equations of elasticity and viscosity are linked together as special forms of the same equation.

The chapter on Statistical Mechanics is rather short, and isolated in its interest. One feels that it might have been wholly postponed till the quantum theory had been discussed in a later volume.

There are short historical notes at the beginnings of each section, which add a further real interest to the book. J. H.

Applied Mathematics for Engineers. III. Differential Equations with Applications. By T. HODGSON. Pp. viii, 320. 13s. 6d. 1931. (Chapman & Hall)

This volume is the natural corollary to volumes I and II, and constitutes a good and varied collection of differential equations and allied mathematics necessary to the student of Engineering and Applied Physics.

Whether there is a real necessity for such a book is to our mind open to doubt. It is inevitable that such a book as this must encroach to a considerable extent on matter which will be put before the students, either in lectures or in engineering text-books during the study of his various engineering subjects. Where such a book can be of real use is in giving him a practical conception of the various mathematical processes with which he is familiar. In this direction the volume under review is to a certain extent successful in an attempt to perform what really can be only fully achieved by personal contact between student and teacher. It provides a good connecting link between mathematical manipulation and engineering science. For the student whose financial status permits, it will be a useful addition to his bookshelves.

J. M. D.

Mathematics ; a Text-book for Technical Students. By B. B. Low. With 409 illustrations and 761 exercises. Pp. viii, 448. 12s. 6d. 1931. (Longmans, Green & Co.)

Those who know the text-books on engineering subjects which D. A. Low has provided for engineering students will be interested to see how his son has met the mathematical needs of the same students, and will find a strong family likeness, with the same wide range and conciseness of treatment, and the same wealth of illustrations and exercises.

The book opens with simple equations, and closes with a chapter (XXVII) on differential equations, all in less than 450 pages, well printed in type of normal reading size, and with the equations and formulae well set out and spaced, but despite this wide range there seems to be no omission of any topic one expects to find, but rather the reverse. For instance, there is a short chapter on Finite Differences (XXIV) containing formulae for interpolation, differentiation, and integration. To accomplish this, an author must needs be concise ; he has no room for digressions ; he is practically compelled to adopt the attitude, " This is how you do it ; watch me and then do likewise " ; and he must rely upon the students' faith in the ultimate usefulness of topics introduced. Within these limits, the explanations are usually adequate, supplemented as they are by profuse graphical illustrations. The matter is grouped into self-contained chapters, the order of taking which is optional. This makes some forward references necessary, and prevents correlated topics from always being in close proximity, but in no other way could such a mass of material be crammed within the covers of a reasonably-sized book. For this book is by far the most comprehensive, and in many ways quite the best,

text-book of mathematics for *technical students* that has so far come to the notice of the present writer, and should be heartily welcomed by teachers.

It is manifestly impossible to discuss it in detail here, but there are some points that deserve comment or need criticism.

In Chapter II, upon Equations, the soluble forms are first dealt with (including Cardan's method for the cubic), but much stress is rightly laid upon graphical and *numerical* methods of obtaining approximate roots of numerical equations. Later, in Chapter XIV, a simple presentation of Newton's method enables the numerical process to be speeded up.

Then follows a chapter on Partial Fractions, the inclusion of which at this stage is difficult to explain or justify.

The chapter on Trigonometry (VI) is the least convincing in the book, and is certainly unsuitable as an introduction to the subject. The six ratios of the general angle are introduced *en bloc* and at once, in terms of "*x*" and "*y*". Identities and formulae are then developed, but what is coming to be known as "*numerical trigonometry*" is lacking in the text (though present in the exercises). Despite the early introduction of the general angle, the fact that there are many angles (and two distinct ones) having the same sine, etc., is not insisted upon, and even in the solution of the "*ambiguous case*" the fact seems to occur as an afterthought.

In the chapter on Series (VII) the idea of convergence is not satisfactorily handled. For instance, when dealing with the sum to infinity of a G.P., incorrect statements appear; the fact that in some cases the G.P. and Binomial series diverge is illustrated by examples "which cannot be true"; in introducing the exponential series, as the limit of $(1+x/n)^n$, the neglect of an infinite number of small quantities is passed over unnoticed, as is also the fact that the series is *always* convergent. While we are far from advocating a thorough treatment of convergence and limits for technical students, the ideas should be presented, and tacit assumptions which merely happen to be true should and could be brought to the light of day—and students capable of getting through this book are capable of both understanding and appreciating these points. Similar criticisms apply to the chapter (XXIII) on Taylor's and Maclaurin's Series, where there is, for instance, nothing to indicate that such a simple function as $\log x$ does not possess a Maclaurin series.

The chapters on Coordinate Geometry (VIII and X, between which a chapter on the geometrical properties of conics is inserted) contain a descriptive account of curves and surfaces, up to conics and quadrics, as far as can be deduced direct from the equations, but coming before the calculus, tangents and normals are not there considered. Some reference to these occurs in the chapter (XII) on Differentiation, and again in the chapter (XXI) on Curvature and Envelopes, but it would have been better to collect a fuller account, say in this latter chapter.

In introducing the calculus, it is doubtful whether enough emphasis is laid upon dy/dx as a rate measurer, although its use as such is exemplified in later examples, both worked and unworked. Integration is introduced in its dual aspect, "inverse differentiation" and "summation", in a manner which seems liable to confound these two distinct ideas, and to obscure one of the most important fluxes in mathematical history, whereby Newton, from his study of rates of change, was enabled to solve the summation problem which had refused to yield to much hard work on the part of his illustrious predecessors. The chapters upon the applications of integration contain much valuable matter, and numerical and graphical methods are given adequate treatment. The chapter on methods of integration contains all that one looks for, but students—not unnaturally—find this a difficult subject, and could be helped by a more systematic correlation and classification of the various methods.

The chapter (XXII) on Complex Quantities is hardly satisfactory. It makes the introduction and manipulation of i appear artificial. It also suffers

from the disadvantage of coming before the sine and cosine series are developed, so that the consequences of the fact that $\cos \theta + i \sin \theta = e^{i\theta}$, which makes De Moivre's Theorem much clearer, and illuminates the whole subject, must be postponed. Nor later, when this result has been obtained, is the close formal connection between the circular and hyperbolic functions brought out.

In the last chapter—the longest in the book—we find all the usual types of first-order equations, and linear equations of higher order with constant coefficients, together with such applications as to struts, shaft whirling, and mechanical and electrical vibrations.

We cannot close this review without referring to the valuable collection of worked examples and exercises, where, besides a sufficient number which are merely practice in working the mathematical machine, an extensive collection of problems arising in engineering is to be found; to the useful table of exponential and hyperbolic functions at the end of the book; and to the printers, publishers and proof-readers, who have produced a book so well printed and got up, and so free from error, at such a reasonable price. W. G. B.

The Elementary Theory of Tensors with applications to Geometry and Mechanics. By T. Y. THOMAS. Pp. ix, 122. 10s. 1931. (McGraw-Hill Publishing Co.)

This work contains selections from a course on Mécanics given during the years 1927-28 and 1928-29 to an undergraduate class at Princeton University. Its primary aim is stated to be an exposition of the more elementary ideas of the theory of tensors and the application of these ideas to geometry and mechanics. The first chapter gives a number of definitions and theorems from elementary calculus and the theory of determinants. Several of the proofs have been given very briefly or omitted entirely, and it is difficult to understand what useful purpose is served by the fragments given. The calculus portion might have been omitted and the references to determinants and matrices expanded. The second chapter, containing the theory of tensors, is the really useful part of the book. However, it is far too short, consisting of only eleven pages, some of which deal with extraneous matter. The section headed *Tensors* actually occupies less than three pages, and is too condensed to be intelligible to the majority of students. The preceding sections dealing with the summation convention and Kronecker's Delta are good. The third chapter deals with Euclidean geometry, treated as a type of physical geometry based on the ideas of congruence and rigid bodies. It is laid down that Euclidean geometry, as treated analytically by means of rectangular cartesian coordinates, is the study of those configurations and their associated magnitudes which remain invariant under orthogonal transformations of coordinates. Emphasis is placed on the Euclidean Principle of Relativity, namely that any system of rectangular cartesian coordinates can be replaced by any other such system. The fourth chapter deals with kinematics, while the fifth and last is devoted to dynamics. These chapters aim at illustrating the statement, made earlier in the book, that many of the equations of mechanics find their simplest and most natural expression when stated in terms of tensors. Footnotes explain how Einstein's theory of relativity has arisen. Some of the matter, such as the twelve pages dealing with the applications of Lagrange's equations to problems, has little or no connection with tensors.

To sum up, we may say that the book contains much that is useful and interesting, but its contents are hardly what we should expect from the title.

H. T. H. PIAGGIO.

Introduction to Vector Analysis, with many fully worked examples and some applications to Dynamics and Physics. By L. R. SHORTER. Pp. xiv, 356. 8s. 6d. 1931. (Macmillan)

This treatise is based on Willard Gibbs' notation for scalar and vector

products, the square of a vector being positive and equal to the square of its magnitude, not minus the square. The scalar and vector products of two vectors might be designated the cosine and sine products respectively, the angle being the angle through which the first would have to be rotated to make it co-directional with the second, so that in the case of two vectors AB, BC the angle is the exterior angle at B of the triangle ABC , and not the interior angle as in the Hamiltonian method. The importance of using the exterior angles which are the angles turned through at each vertex in walking round the triangle is well exemplified in geometrical applications, and perhaps specially in spherical trigonometry, the fundamental equations of which are immensely improved by their use. Thus, if the formulae relating to a triangle ABC are expressed in terms of the sides (a, b, c) and exterior angles (α, β, γ), the interchange of sides and angles requires no readjustment of signs, and the functions of the half-angles and of the half-sides are expressible in exactly similar manner, using $2s = a + b + c$, $2\sigma = \alpha + \beta + \gamma$. The area of any polygon is also most simply given in terms of σ , being $2s^2(\pi - \sigma)$. The formulae for the square of the side of a triangle come at once: thus

$$c^2 = (a + b)^2 = a^2 + b^2 + 2ab \cos \gamma$$

in a plane triangle, and

$$\cos c = \cos(a + b) = \cos a \cos b - \sin a \sin b \cos \gamma$$

in a spherical one, are automatic scalar expansions.

The first two chapters of the book deal with addition of vectors, with numerous examples and their solutions, the latter being perhaps at times a little too laboured, rather obscuring their intrinsic beauty and simplicity; but on the whole very helpful, notwithstanding. Chapters III and IV deal with multiplication, and examples, with solutions: very valuable chapters. Chapters V and VI introduce differentiation, and the Heaviside notations, *grad*, *div* and *curl*. They require very close attention, and are hard reading for beginners, but are very carefully explained and illustrated by worked examples.

The seventh, and last, chapter is devoted to applications of the vector methods to Kinematics and Kinetics and to line and surface integrals, and is very comprehensive. It does not always convince one that this vector method is the best mode of approaching some of the problems, but it gives the methods and leaves the reader to choose what will help him most. The terms *gradient*, *divergence* and *curl*, applied to vector differentiation, occur so frequently nowadays that a handy book like this, explaining them, and how to use them, will fill a great want.

At the end is a list of equations (in No. 8 of which is a misprint v_1 for r_1) which should be useful for reference; and the book finishes with a good index, by which and by the excellent Table of Contents at the beginning, any required part may be found.

ALFRED LODGE.

By Graph to Calculus. By E. T. CHISNELL. Pp. viii, 86. 2s. Or in two parts: Find the Formula. 10d. From Formula to Calculus. 1s. 3d. 1930. (Harrap)

It is sufficient to give a short description of this little book and to say that Professor Sir T. Percy Nunn writes an introductory blessing. The general method of Part I is to obtain data for a problem from laboratory, workshop or classroom; to plot the graph therefrom; to recognise the type of the graph and from it to find the working formula. The underlying idea is the invention and interpretation of formulae, rather than their mechanical application. The purpose of Part II is to extend this idea and to lead by way of the graph to a simple understanding of the Calculus. The book is specially intended for technical, central and senior schools. It is attractively printed with good diagrams, and there are answers to the examples.

N. M. G.

The Methods of Statistics. An Introduction mainly for Workers in the Biological Sciences. By L. H. C. TIPPETT. Pp. 222. 15s. 1931. (Williams & Norgate)

Perhaps in no other subject of study has the general format of the text-book changed so completely in recent years as in statistics. We have on the one hand the standard treatises of Yule, Bowley and others in which the subject is developed along more or less standard lines, with special sections, labelled for the advanced student, and often only too restricted in scope, dealing with the nature of the exact theory of sampling. On the other, the stimulus given to the subject by the researches of R. A. Fisher, brought about, to the gain of experimental science, by the close association of the mathematician with the biological worker, led to the appearance in 1925 of *Statistical Methods for Research Workers*, a book which invites comparison with the one under review, since it too is addressed mainly to workers in the biological sciences. In spite of the non-mathematical character of Fisher's book, more apparent, be it added, than real, the average student has found its study very difficult, and one naturally looks in a new text-book, especially one that follows Fisher to a large extent in method, for a more elementary exposition of the simpler portions of the subject, and for a fuller and more extensive illustration by means of examples, a part of a text-book that does much to clarify a subject to the reader. Our author has felt impelled, in his own words, "to present a single system of statistics, so that a reader with little previous acquaintance may obtain a good working knowledge and understanding of the methods available". The result is a very readable book, which errs, if at all, on the side of attempting too much within the compass of a little over 200 pages.

The various chapters deal with: frequency distributions and constants, theory of random sampling, goodness of fit and contingency tables, small samples, analysis of variance (the development of which is given a chapter to itself although it permeates the rest of the book), correlation and its sampling distribution, non-linear regression, further theory of errors and principles of experimental arrangements, and finally multiple and partial correlation and regression. No tables are included, but reference is made so often to tables existing elsewhere, most of them in another text-book, that we feel strongly that a book like this one should either stand on its own feet and supply the necessary tables, or preferably that a useful companion volume to all books on statistics, which need be neither very big nor very costly, should be prepared from existing sources to contain the half-dozen or so most essential tables. The treatment of the more elementary portions of the subject is good, but the student would welcome, as an illustration of method, examples of the calculation of the mean and standard deviation from, say, a sample of ten ungrouped values. He will tend, after patiently accepting certain mathematical formulae for the moment coefficients, to be appalled by the example of Table VI, where every conceivable operation relating to the fitting of a normal frequency curve is given him to swallow at one gulp. Prior to the study of the Binomial and Poisson series the reader is only told that a mean is obtained by adding up all the values of the variate and dividing by their number, and yet without further explanation he is presented on pp. 32 and 35 with statements of the means from grouped data. Proofs of propositions have not been given, we are told, unless they are easy, but we are left to wonder why the student who is unable to follow the proof of the formula for the standard error of a mean is assumed to be able to understand without much explanation the much more difficult question of the mathematical symbolism, for e.g. the s th. moment coefficient from grouped data (p. 38). The book is, in fact, not entirely elementary in character, and some parts of it will prove hard reading.

The general impression left on the reviewer is that the book has been written too hurriedly, so that a number of minor errors have crept in, while

the descriptive matter is often loosely worded, and terms of the author's own are used without a precise attempt to define them beyond all possibility of doubt. The grammar, even, is not always above question, and the reader is occasionally troubled by an "it" or "them" that he has difficulty in tracing to its proper context. The very first sentence may be contested. If by "subject" is meant statistics, it should be emphasised that Whittaker and Robinson's book, *The Calculus of Observations*, deals with the much wider subject of numerical mathematics, of which statistics is only a part.

Looseness of definition may be illustrated by "infinite population", which is assumed "so large that its composition is not altered by the abstraction of any finite number of samples" (p. 44); looseness of statement by the definition of coefficient of variation (p. 25), which is the standard deviation expressed, not "as a ratio to", but as a percentage of, the mean; by the normal curve as a limit to the binomial (p. 35), where p and q are not necessarily equal to $\frac{1}{2}$; by the statement (p. 38) "Sheppard's corrections are" instead of "the moments incorporating Sheppard's corrections are"; and by the term "mean variance" (Chap. X) for something that is certainly not a mean, but is rather a single sample estimate of the variance. Obvious errors occur on p. 25 in the definition of quartile deviation, where mean should read median; on p. 53, where, dealing with $(n+1)$ samples, we compare, not "them", but " n of them", in turn with one, a control; on p. 66, where the P of the table does not agree with that of the text; p. 128 (foot) and Table XXXII. the origin arrays are not both bounded by heavier lines; and on p. 147, last line, where σ_x should read σ_x^2 .

J. WISHART.

Einführung in die Höhere Mathematik. By H. VON MANGOLDT. Fifth and sixth edition, revised by K. KNOPP. Pp. xv, 585. Rm. 20. 1931. (Hirzel)

This new edition of von Mangoldt's well-known work has been completely revised, extended and brought up to date by Knopp. In the revision the general aim of the treatise has been kept always in mind, to produce an account at once rigorous and readable of the elements of Analysis. The well-balanced treatment and clarity of exposition which were such admirable features of Knopp's *Infinite Series* proved that the author was as well able to expound a subject as to extend its boundaries. In the present work the same balance and clarity pervade the whole.

In spite of its many merits, it is hardly probable that the book will find a large number of readers in this country. It is, we think, a book to be placed in the hands of an intending specialist, who has a fair acquaintance with the elementary technique of the Calculus and is ready for a completely rigorous exposition. But such students will as a rule prefer at this stage an exposition in English, and will of course turn at once to Hardy's *Pure Mathematics*. Teachers will find many things of interest and of value in von Mangoldt, however, and anyone learning German for mathematical purposes would be well advised to tackle this volume.

The second and third volumes are concerned with the Differential and Integral Calculus respectively, so that the volume under consideration is devoted to the task of laying firmly the foundations upon which the superstructure of the Calculus is to be reared. Algebra—Permutations, Combinations and Determinants—occupies the first hundred pages. Then we have chapters on rational, real and complex numbers. Two long sections deal with algebraic geometry, constructing coordinate systems in two and three dimensions, with applications to the straight line and plane. Considerable space is given to sections on variables and functions, and on limits of sequences and of functions; the volume concludes with two shorter chapters on the theory of sets and continuity.

In a book like this a great deal of novelty is not to be expected. The function of such a volume is to provide a clear and logical account of well-

established methods and results. But in one particular domain, the methods adopted are, we believe, relatively unfamiliar to teachers of Analysis in this country. We refer to the treatment of limits of sequences ; this is based on the properties of "null sequences". After defining a number-sequence $\{x_n\}$, a null sequence is then defined to be one for which, given any positive number ϵ , there is an integer n_0 , such that

$$|x_n| < \epsilon \text{ for all } n \geq n_0.$$

Examples of such sequences are given, with some of the more obvious properties. This introduction is placed at the end of the section on rational numbers. When we come to the section on limits, we find the definition that if a sequence $\{x_n\}$ and a number ξ are so related that the sequence $\{x_n - \xi\}$ is a null sequence, then the sequence is convergent, and ξ is the limit of the sequence. It is possible that this method, which does not differ in essentials, but merely in presentation from the more usual treatments, might be a real simplification in the teaching of the theory of limits. It is, however, not quite clear what position would be found for the upper and lower limits of sequences in such an account. They are mentioned in the chapter on the theory of sets, but little stress is laid on them. It would seem from a comparison with Knopp's *Infinite Series* that in the three volumes of von Mangoldt's book, only the really elementary portions of the theory of series are to be used, and for this the treatment given will be sufficient.

A word of praise must be added for the paragraphs—too few in number, but delightfully lucid—on the elementary inequalities.

Operational Methods in Mathematical Physics. By HAROLD JEFFREYS. Second edition. Pp. viii, 117. 6s. 6d. 1931. (Cambridge)

The first edition of this work—No. 23 of the *Cambridge Tracts*—was the subject of an exhaustive review in the *Gazette** by Professor Carslaw, with remarks by Dr. Bromwich and Dr. Jeffreys. It would be foolish to attempt to add to the account there given of the subject-matter of this tract, and all that is here necessary is an expression of pleasure that a second edition should have been called for only four years after the appearance of the first, and an indication of the slight changes that have been made. The chapter on Bessel functions has been rewritten, and a discussion of the transmission of a simple type of telegraphic signal along a cable has been added. The first two chapters, which deal with the fundamental ideas of the Heaviside operational calculus and the interpretation of the results through the theory of complex integration, have been revised and amplified. The exposition is thereby improved at a most important point, for once the leading ideas have been firmly grasped, the rest of the tract presents no formidable difficulties to the reader.

Dr. Jeffreys quotes Heaviside's dictum, that "even Cambridge mathematicians deserve justice". It would be bald justice to say that Dr. Jeffreys is an admirable expositor ; we feel certain that it is also superfluous.

Elementary Hyperbolics. By M. E. J. GHEURY DE BRAY. Vol. I. Hyperbolic Functions of Real and Unreal Angles. Pp. xi, 351. 7s. 6d. Vol. II. The Applications of Hyperbolic Functions. Pp. xii, 209. 7s. 6d. 1931. (Crosby, Lockwood & Son)

The author of *Exponentials Made Easy* has now fulfilled the promise implied in that work of a book on similar lines dealing with imaginary numbers and hyperbolic functions. It is not necessary to say a great deal about the second volume of the present work ; it contains applications of the hyperbolic functions to maps, solution of equations, strength of materials, catenary suspensions, and the propagation of electric currents in long conductors. These applications are carried out with force and clarity, and should be very valuable.

* *Math. Gazette*, xiv, 216.

But readers of the *Gazette* will be chiefly interested in the first volume, wherein the author deals with the imaginary number and the hyperbolic functions, starting from scratch. Let us say at once that this is a volume which may be of great assistance to a beginner; some of it is amusing, much of it is interesting, and all of it is written with a sincerity and enthusiasm for which we have nothing but admiration. There are, however, many faults, and unfortunately most of these seem to be the result of woeful ignorance. On p. 104, the author talks of "... a way to make a simple fact understood by the most unprepared mind, and to impress it so forcibly that it cannot be forgotten again, and that is what the Author's job is! The job of a mathematician is quite different". Now for the last three hundred years, the mathematicians in this country have, in general, been teachers, and surely their accumulated experience of methods of teaching mathematics ought not to be neglected quite so contemptuously. It may be pedantic to point out the ill-accord between p. 7, l. 19, "using the ordinary rules of algebra,

$$\sqrt{-a} = \sqrt{a \times -1} = \sqrt{a} \times \sqrt{-1},$$

and the agnostic remark ten lines earlier that the square root of a negative number "is not, however, imaginary in the ordinary interpretation of the word, for we cannot form any idea of what it is like, we cannot imagine what it may be...". But it can hardly be pedantic to be surprised at finding on p. 89 a numerical example with an answer "2.024 hyperbolic radians", when we have learned on p. 87 that "there is no such unit as a hyperbolic radian". The author may have a reason for his very old-fashioned derivation of the exponential series from the binomial theorem, but reverence for the antique can be carried too far: at the bottom of p. 35 he very neatly summarises the objections to the " \tan^{-1} " notation, and we turn the page in the hope of finding the "arc tan" introduced, but instead we are informed that the \tan^{-1} notation "has become permanently established".

At the risk of over-emphasising the weaknesses of the first volume, we make one more quotation. Bearing in mind the remark on p. 104 that

$$\sinh(\text{dog}) = \frac{1}{2}(\epsilon^{\text{dog}} + \epsilon^{-\text{dog}}),$$

we find on pp. 109-10 this paragraph—the italics are ours:

$$\sqrt{\sec 2\theta} = \sqrt{\cosh 2u} \quad \text{or} \quad \sec 2\theta = \cosh 2u.$$

(Do not make the blunder of writing this in the form $\sec \theta = \cosh u$, by dividing by 2; as we have seen in Chapter IX., u increases rapidly while θ increases uniformly, so that it is not even correct to suppose that, if $2\theta = 2u$, then $\theta = u$ or $\frac{1}{2}\theta = \frac{1}{2}u$, still less to suppose that, because a function of θ equals a function of u , the equality will persist when θ and u are halved or doubled. Even in simple trigonometry, remember, if $\sec 2\theta = \tan 2\phi$, for example, it is not true that $\sec \theta = \tan \phi$.)

We leave it to the reader to judge how well this will "make a simple fact understood by the most unprepared mind".

The printing of the two volumes is good, though it is a pity that in printing a sequence of equalities it has been necessary, doubtless in order to save space, to get two and even three equality signs in one line. The numbering of formulae is ingenious; thus the three new formulae on page 110 are numbered (110a), (110b) and (110c), and this certainly facilitates reference.

It may be thought that we are applying standards of criticism which would only rightly be applied to books meant definitely to be for students of mathematics. In reply, it may be said that it is somewhat difficult to tell for what class of reader the book is intended, and again, that since the author has found it so frequently necessary to warn his readers about the shortcomings of mathematical teaching, it may properly be the duty of a teacher of mathematics to endeavour to return the compliment. We have tried to do so without malice by pointing out some of the faults which mar an interesting and stimulating work.

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